SCIENCE

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THE INTELLECTUAL CONDITIONS FOR EMBRYOLOGICAL SCIENCE.

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NATURAL HISTORY AND NATURAL KNOWLEDGE.

THE definition of science as the analysis and classification of facts leads the philosophical spokesmen of modern science to believe that an embryological account of thinking men is impossible, because it leads them to believe there is a chasm which is intellectually impassable between the facts of physics and the facts of consciousness.

Since the minds and senses by the aid of which we make scientific discoveries are generated from eggs, the progress of embryological science must bring us around sooner or later to the old question: What is science? What is it to know a thing?

In this paper I shall show the fitness of biological science for helping us to reconsider this great question.

1. *May it not be that we understand a thing when we can tell what it means, and use it?*

Philosophers tell us we understand a thing *when we comprehend it,* but it is my purpose to ask whether the progress of biological science may not lead us to think, with Berkeley, that we understand a thing *when we can tell what it means and use it,* and whether this definition of science may

not help us out of the paradoxes of philosophy, and make the way clear for an embryological account of thinking men.

2. *The problem of knowing.*

Our sensations and thoughts and feelings have not taken place anyhow and at random. They have been so related in the past that one has been a sign which has led us to expect others, which have always come about as we expected if our knowledge has been sound and accurate.

When they have thus come about, we have known that *this has not been our doing*. We have known too that it is because it has not been our doing that natural knowledge has been useful to us.

One of the most practical questions that man can ask is this: When, and how far, is our experience a sound basis for confidence in things that we have not experienced—such things, for example, as the animal life of the Cambrian sea, the molecular constitution of matter, and my own embryonic history? Since an answer to this question has been included in past knowing, it must also be included in an account of knowing. It is this question that physical science undertakes to answer by scientific discovery, but the biologist must ask a still more difficult question: How do living beings come to do unconsciously, and without knowing it, the things that are to their advantage? How does a living being get safely through all the chances and changes of life without needing to run its nose into every danger before it avoids it? How do some men learn, from a single experience, what others fail to find out after a lifetime of experience?

3. *Our biological education begins at an early day.*

No institution, no period in the history of science, no stage in intellectual development, can lay claim to the beginnings of biological science. They are to be sought long before our entrance into laboratories; long before the beginnings of book-learning. Even before we learned articulate speech, the teacher whom the poet has called the grand old nurse took us upon her knee and began the wonderful story of nature for our delight and profit and instruction; that story to which there is no end; in which each chapter, as fresh and new as the first, adds new meaning, new usefulness to all we have been told.

Part, at least, if not the whole, of our early education was biological. We laid the foundations of anatomy and experimental physiology when we learned, through repeated scientific experiments, that it is through eyes that we see, through ears that we hear, through hands that we touch, and that it is good to see and hear and handle things. We were no doubt led, slowly and gradually, through innumerable scientific experiments, to the discovery that, among the changes that go on in nature, some are of peculiar interest and importance to us; and we thus come to set apart in our minds, from among the things of which our senses tell us, certain ones which seem, because of their clear relation to our comfort and discomfort, and because of the quickness with which we learn how to make use of them, to pertain to ourselves, and to constitute our bodies, as distinct from the world around us, which we are thus led to set over against ourselves, as a not-self. It seems to me that it is in this way that we lay a foundation, in the conception of a living body, for all later study of biological science, and no naturalist can doubt the great and permanent value of this conception; yet there is no more fruitful source of paradox and contradiction and absurdity than the words in which we attempt, at a later stage, to describe this scientific discovery; for while a scientific discovery is part of the language of nature, our words

are, unfortunately, an inheritance from the language of scholasticism.

4. *Education is often unconscious.*

One may be educated without knowing it. The teacher who guides instead of driving is nearest the method of nature. The best of all training is that which is acquired with least effort, and some of the choicest fruits of intellectual activity have come when effort and self were lost in the inspiration of creative genius.

The untrained muscles of the infant are educated through exercise, but it is not in self-consciousness that the child and the kitten and the colt and the calf delight in frolics and gambols and sports and games. It is only after the human infant has spent weeks in experimenting that it acquires the useful art of moving its eyes together, and of seeing objects single and solid, instead of flat and tremulous. I do not know whether the child is conscious or unconscious of this lesson in the physiology of vision, but it is, assuredly, not through induction from particulars and deduction from laws that the nutritive and nervous changes come about, through which the muscles of the eyeball become coordinated, yet these are educational changes. So far as education is shown by doing the things that are advantageous, and in avoiding those that are injurious, the ancestral rhizopod, which extends its pseudopodia under the stimulus of fit food, and retracts them on the approach of danger, is educated, for our biological education begins long before our birth, and we are born educated. It is this truth, no doubt, which has led some to the strange notion that life is memory.

5. *The things we do most easily, or most naturally, are not always the wisest things.*

The history of our minds, like that of our bodies, has been such that the things we do most naturally are not in all respects the best for our present needs. Just as there are bodily parts which, while fitted for past conditions, are no longer useful, and just as we have natural impulses and appetites which now call for repression, so it is also with our minds; for we are in continual danger of a logical fallacy which it is the peculiar work of natural science to correct, since it is an incidental result of our natural history. This is the fallacy known to logicians as the fallacy of the undistributed middle—the fallacy which consists in mistaking a part for a whole.

6. *The fallacy of the undistributed middle is constitutional.*

Our bodies are so constituted that an action which is at first performed with difficulty becomes easier with each repetition, while departure from established custom at the same time grows harder. It is this peculiarity which fits our bodily frame for improvement by practice and training. In this, our minds are like our bodies, for a path which our thoughts have once traversed becomes easier with each new venture, while it grows harder for us to consider what lies beyond the borders of this path.

The facts of nature do not all interest us equally. Some are more attractive to us than others, and we must specialize to make progress in knowledge, so we are continually and unconsciously fixing attention upon some part of nature, for some purpose of our own, and considering it 'in itself,' to the neglect of that which does not interest us, nor seem to concern us.

Our minds, as they have come to us in course of nature, are so constituted that, when we consider a part as if it were the whole, we are in danger of forgetting that it is but a part and not the whole; and if we make this mistake, we may be led into opinions which seem to be the logical conclusions of sound reasoning when they are nothing more than new illustrations of the

threadbare fallacy of the undistributed middle.

7. *Philosophical agnosticism comes from mistaking a part for a whole.*

When, for some purpose of our own, we become interested in a part of nature, neglecting, for the time, as of no interest to us, its interrelations with other things, we may fall unconsciously, from the very nature of our minds, into the belief that what we have treated as if it were independent of the rest of nature, and complete in itself, is really independent and complete. Thus we come to regard mental abstractions as independent things, and then, finding that our abstractions have no independent being outside our minds, we ask the absurd question whether the real world of nature is anything but an abstraction and a chimera of our fancy, and set ourselves to making systems of philosophy to pull us out of the quagmire of agnosticism into which we think we have fallen.

Berkeley shows that it is because we call all sheep and all crows and all triangles and all numbers by generic names, that we think we can know a generic sheep and a generic crow and a generic triangle and a generic number—that is, a sheep and a crow and a triangle and a number which are not individual and particular sheep and crows and triangles and numbers; and he believes that it is nothing but language which makes us so ready to mistake abstractions for independent things, and then to think that because no real thing exists abstractly we can never know anything as it really is; and he shows that 'we need only draw the curtain of words to behold the fairest tree of knowledge, whose fruit is excellent, and within the reach of our hands.' So firmly rooted in our minds is the notion that abstract words stand for *things as they really are,* that Berkeley, who only asks us to use our utmost endeavors to obtain a clear view of the things we would consider, 'separated from all that dress and encumbrance of words which so much contributes to blind the judgment and divide the attention,' is commonly held to deny the reality of *things,* because he denies that any real thing exists abstractly.

Tyler traces our habit of mistaking abstractions for independent things, and the doubt of the reality of *things* which arises in the mind of the philosopher when he discovers that no real thing exists abstractly to the primitive culture of savages, and it is, no doubt, because there is still much of the savage in us all, that we try to distinguish the appearance of things from those things in themselves of which the appearances are thought to be the ghosts.

May we not trace still farther back the habit of mistaking abstractions for independent things, and ask whether it may not be an unfortunate incidental result of that fitness of living beings for education which is older than the trilobites?

It is not the value nor the reality of generalizations, but their independent, or abstract, reality, that is called in question. A generalization is as real as a pain, and, like a pain, it may have the greatest value, and call our attention to other real and important things which might have escaped notice, and it may thus help us to foresee or direct nature.

If the pain were not my pain it would not be at all; yet, while its being is relative to me, this relation to me is not all the being it has. No fact is more certain than that I do not make my pain, for if it were my doing it could not call my attention to unnoticed things, nor have any value as a warning of danger. Is it not ignorance of this simple truth which has led some to think that our pain is our own doing, and that we need only stop doing it to make an end of it?

All I know about the trilobites and the moons of Jupiter is relative to me; yet the trilobites were real millions of years before any naturalist knew them, and the moons of Jupiter would, no doubt, still be real, even if all life should come to an end upon earth.

8. *Our bodies are real, but their reality is in their interrelations with our environment.*

The child's discovery that its body is of peculiar interest and importance to it, and peculiarly within its control, is a real scientific discovery. Living things are real things, and we can never know too much about them; but their reality is in their interrelations with the rest of nature, and not in themselves, nor in their relations to us. Surely this is good sense and good science. No physiologist who studies the waste and repair of living bodies, no naturalist who knows living beings in their homes, no embryologist who studies the influence of external conditions upon development, can, for an instant, admit that living beings are self-sufficient or self-sustaining, or that their being is in themselves; for the line we draw, for better study, between living beings and the external world, is not one that we find in nature, but one that we make for our own purposes.

The external world of a living thing is as much a part of it as its histological structure. If the environment of its body, or of any cell within its body, were different, neither cell nor body would be what it is, and if they had no environment they would not be at all, for neither eggs nor seeds nor desiccated rotifers exist abstractly. A self-sufficient and self-contained living thing is as fabulous as a griffin or a centaur, but no naturalist thinks for an instant that this truth casts any doubt upon the real existence of living things.

If the being of a living thing is in its interrelations with the world around it, as Berkeley tells us it is, and not in its interrelations with us, as the philosophers tell us it is, is it not clear that we can never hope to know all there is to know about it? But is it not equally clear that, so far as we do know it, we know it as it is?

Does the responsibility for the notion that we can never know a living being as it really is rest upon the shoulders of the naturalist who knows that its being is dependent and relative? Is it not rather to be laid to the charge of the philosopher who believes in its abstract or independent existence, and is led to doubt its reality by the discovery that abstractions have no independent existence?

Locke reminds us that "we see and perceive some of the motions and grosser operations of things here about us, but whence the streams come that keep all these curious machines in motion and repair, how conveyed and modified, is beyond our notice and apprehension; and the great parts and wheels, as I may say, of this stupendous fabric of the universe may, for aught we know, have such a connection and dependence in their influences and operations one upon another, that perhaps things in this our mansion would put on quite another face, and cease to be what they are, if some one of the stars or great bodies, incomprehensibly remote from us, should cease to be, or to move, as it does. This is certain: things, however absolute and entire they seem in themselves, are but retainers to other parts of nature, for that which they are most taken notice of by us. Their observable qualities, actions and powers are owing to something without them; and there is not so complete and perfect a part that we know of nature, which does not owe the being it has, and the excellencies of it, to its neighbours; and

we must not confine our thoughts within the surface of any body, but look a great deal farther, to comprehend perfectly those qualities that are in it."

9. *The being of things is real, but is it in themselves, or in their interrelations?*

Is it as a self-contained and self-sufficient being, or as part of the universe, that the stone illustrates the law of gravitation?

When Sir Isaac Newton made his speech about the child and the pebble: "Did he mean," asks Dr. Holmes, "to speak slightingly of a pebble? Of a spherical solid which stood sentinel over its compartment of space before the stone that became the pyramids had grown solid, and has watched it until now! A body which knows all the currents of force that traverse the globe; which holds by invisible threads to the ring of Saturn and the belt of Orion! A body from the contemplation of which an archangel could infer the entire inorganic universe as the simplest of corollaries! A throne of the all-pervading Deity, who has guided its every atom since the rosary of heaven was strung with beaded stars!

"The divinity student honored himself by the way in which he received this. He did not swallow it at once, nor did he reject it; but he took it as the pickerel takes the bait, and carried it off with him to his hole (in the fourth story) to deal with at his leisure."

10. *May not the notion that our minds are in our heads be due to the fallacy of the undistributed middle?*

Our welfare and our existence depend upon the soundness and safety of our brains, and knowledge of real brains and their functions is of the utmost value and importance, but would it have any value if, knowing only the appearance of brains in our minds, we were altogether put off with false appearances, and could never know brains as they are in themselves?

If the being of a living brain is not in itself, but in its interrelations with nature, we do know brains as they really are when we discover these interrelations; but if the being of a brain is not absolute and independent, but dependent and relative, what are we to think of the notion that our minds are shut up inside our heads? May not this also be an illustration of the fallacy of the undistributed middle? My mind to me a kingdom is, but I find no reason to think this kingdom is a microcosm—a little world set over against the great kingdom of nature. My kingdom is the great universe itself, the starry heavens, and the geological history of the earth, and everything else I know, and my mind grows as more and more of nature becomes mine by right of discovery. So far as I know the Ichthyosaurus and the rings of Saturn, these things are in my mind; and if the things I know were really shut up in my skull, these things would be inside my skull; but there is no room there for real whales and real megatheriums, so philosophers tell me I can never know anything as it really is, because the only universe I can think of or consider is the one I know.

Stone walls do not a prison make, nor iron bars a cage. May we not owe to the fallacy of the undistributed middle—to our useful ability to fix our attention upon a part of nature, and to temporarily neglect that which does not for the time interest us nor seem to concern us, and to the carelessness which permits us to think that what we have considered by itself for our own purposes is really self-contained and self-sufficient—may it not be to this that we owe the notion of a mind shut up in a head, and knowing nothing but the dissected and distorted shadows which the unknown and unknowable real world casts on the walls of its prison through its narrow and grated windows?

11. *Illusions and hallucinations do not show that the world I know is unreal, nor do they show that its reality is relative to me.*

Deceptions and illusions and hallucinations are not unreal. They are matters of fact of which the physiologist and the pathologist and the physician are finding out the meaning, and finding too a way to make use of this meaning, by scientific discovery, while common folks mistake their meaning, just as we mistake the meaning of other matters of fact when we think we know more than we have found out.

Comprehension is the gathering in of generalizations into a hypothesis, but while any plausible hypothesis may satisfy idle curiosity, it has no scientific status unless it leads to the discovery of facts and the control of nature.

When the ignorant man who has lost his foot feels the sensation which he has learned to call pain in his toes, he says his foot is uneasy in its grave. When the learned philosopher tells him his pain is an illusion, he may justly declare that he knows his own feelings better than any one else, however learned. The pain is real, but when he satisfies himself with the notion that his foot is uneasy, he mistakes a hypothesis for a fact, like the philosopher, while the man of science discovers that the sensory nerve is irritated somewhere else than at its endings in the toes.

12. *Instead of showing that we can never know anything as it really is, may not the notion that knowledge is comprehension be a new illustration of the fallacy of the undistributed middle?*

We comprehend things when we know them, but it does not follow that when we comprehend them we know them, for knowledge may be comprehension and something more.

The resemblances between things are summarized by classifying or comprehending them, but Locke has reminded us that knowledge is the discovery of resemblances *and differences.* So far as we know nature, it exhibits universal order in endless diversity; not order here and diversity there, but order in diversity. Can we know any two things are alike without knowing they are different? We may, for some purpose of our own, fix our attention upon the order of nature, neglecting the diversity, but things do not cease to be because they do not, for the time, seem to concern us.

Are the order of nature and the diversity of nature either two things or one thing seen from two standpoints? Are they not rather two narrow and imperfect views of the natural world which lies before our eyes? Have we any way to find out either the unity of nature or the diversity of nature except scientific discovery? May not the notion that while we discover the laws of nature, we deduce from these laws the diversity of nature, and our control of nature, be an illustration of the fallacy of the undistributed middle? Is a scientific law anything more than a summary of past experience, *joined to confidence in the continuity of nature?* Do we ever know that we can foresee or control nature, even in repeating the simplest scientific experiment, until we have succeeded?

13. *Biological science is peculiarly fitted for calling to our attention the diversity of nature.*

While analytical science is making marvellous revelations of the order which pervades the apparent disorder of nature, showing us, by the method of analysis and generalization, the most astonishing proof of order and regularity in the course of events which had seemed to be chaotic, biological science is continually recalling to our attention the diversity of the statistical data, and making equally marvellous

and equally instructive, revelations of the inexhaustible variety of nature. We talk about humanity, but we know and deal with Peter and Henry and Thomas and Black Jim and Yellow John. We need proper names for all the animals that we are well acquainted with. The zoologist tells us about the genus *Equus,* but if he has any practical dealings with horses, he never says one horse is the same as another, or even that a horse is the same to-day as he was yesterday, for even if he be neither sick nor lame nor hungry, he is one day nearer the end of his usefulness.

The botanist talks learnedly of *Chrysanthemum indicum,* but the florist sells golden wedding and ivory and fair dawn and snow queen and hundreds of others. For many scientific purposes it is necessary to give proper names, or designating numbers, to seedling plants, and it may be that if the chemist were dealing with individuals, instead of averages, he might need proper names to tell to others his discoveries about molecules and atoms.

14. *Are identity and diversity absolute or relative?*

To-day's sun is the same as yesterday's, yet the changes which go on in the sun, from day to day, are, no doubt, violent and rapid beyond our utmost means of measurement or expression. We say to-day's sun is the same as yesterday's when we are interested in the dawn and the daylight, and in the flight of time, and in the change of seasons, and in the transit of Venus, and in the stability of the solar system; but we say it is not the same when we are interested in sun-spots, and in the fall of meteorites, and in combustion and the dissipation of energy. When we say the solar system is stable, we do not mean that it is really stable. We only mean that the course of its progress from some past condition to some future condition has no obvious practical relation to our own affairs.

We seldom lose sight of the diversity, or individuality, of familiar living things in our interest in their resemblances. We do not say one horse is the same as another between the shafts. We say he is as good as another, or will serve, or that he is the same *substantially,* meaning, by these words, the *same substantially,* or *the same in substance,* that, while he is not the same, we will accept him as a substitute; but no one with worldly wisdom trusts the strange horse, even so far, before he has tested his opinions, and those of the horse dealer, by scientific experiment and verification.

Biological science has peculiar fitness for guarding us from the fallacy of the undistributed middle, and for teaching us that it is only through verification that guesses become knowledge, because its subject matter lies midway between those 'exact' sciences in which we are told that figures cannot lie, on the one hand, and, on the other, those social and political sciences which show us continually how easily one may lie with figures. When we have verified a hypothesis so often that we are 'satisfied,' we call it a 'law of nature,' and we build as firmly upon it, and trust to it as implicitly, and govern our actions by it as unhesitatingly, as if it were certain, and in all that concerns our conduct we make little or no difference between it and certain knowledge. In this, experience is continually demonstrating our wisdom, but if the discovery that hypotheses have no independent existence leads us to believe that we can never know the real world of nature, is it not time to reexamine our notions?

The laws of nature are real, but their reality is not independent nor absolute, because the unity of nature is unity in diversity, and diversity in unity.

If the views that are here advanced—views that are in no way original with me—are accepted; if the reality of the nat-

ural world is in the interrelations between things, and not in unknown and unknowable things as they are in themselves; does it not follow that scientific discovery is the only way to learn the differences between things, just as it is the only way to learn the resemblances between things? When we say two things are the same, must we not also say what are the relations with reference to which they are the same? When we say they are different, must we not also say what are the relations with reference to which they are different? Is there any way except scientific discovery to find this out?

15. *The biological problem of species.*

Fifty years ago many naturalists thought that all living things of a kind are fundamentally and absolutely alike in certain specific characters, and that it is only in characters that are not specific that they differ; but more exact study has failed to show us, in any living being, any characteristic whatever which does not exhibit diversity from others of its kind, as well as resemblances; for the notion that certain characters are generic, while others are differential, is an illustration of the fallacy of the undistributed middle, as is also the attempt to analyze living beings into *characters*.

After the long controversy between those who asserted the immutability of species, and those who declared that species are mutable, seemed to be happily ended by the scientific demonstration that species have a natural history, there arose a new school of naturalists, who asserted that species have no existence in nature because no two living beings are identical in any respect whatever. At the present day, many naturalists are returning to a modification of the old notion of species, and are teaching that while the mutability of species is due to changes in the interrelations between living beings and the world around them, stability is inherent in the living beings, *as they are in themselves* by birth.

If the view which is here advanced be correct, the specific stability of the individuals of a species is real, and as independent of us as the stability of the sun in the heavens, but when we say the individuals of a species are alike, we must also say what are the relations with reference to which they are alike, for the stability of species and the mutability of species are not two facts, nor the same fact from two points of view, but two narrow and imperfect views of the same fact.

Thus, for example, individual sheep are alike for certain purposes of the zoologist and the paleontologist. They are alike to the embryologist and to the anatomist, and to the physiologist, so far as these scientific students are not concerned with their differences. They are, no doubt, alike to the hungry wolf, and to the geese that graze in the same pasture—to their competitors and enemies in the struggle for existence. They are alike in their sexual affinity, so far as there is no sexual selection. They are alike in the physiology of reproduction, and in their physiological activity in general, so far as they do not differ in fertility and in constitution. On the other hand, they are different to the stock-breeder, and to the shepherd, to the shepherd's dog, to their lambs, and, no doubt, to each other.

As we learn more about sheep, we learn more about their identity and more about their diversity, but this does not show that the identity and diversity are in us and not in nature. It only shows that neither the identity nor the diversity has any independent existence in nature abstracted from the living beings.

16. *Are inheritance and variation two processes, or two partial and imperfect views of the same process?*

If no two individual living beings are alike; if the stability of biological types means that the aberrant have been exterminated in the struggle for existence, and if the modification of a type is an indication of a change in the standard of extermination; are not inheritance and variation two partial and imperfect views of the selective process? When the embryologist seeks in the germ for the material basis of inheritance, and for the mechanism of variation, is he not searching for something which has no independent existence? Must he not seek, in the interrelations between living beings and their environment, and not in the living beings *as they are in themselves,* for that of which he is in search? Do not they who think that natural selection must be supplied with the raw material by a mechanism for variation before it can *do* anything, both personify the selective process and forget the diversity of nature?

17. *Does physical analysis give an adequate account of the organization of living bodies?*

Physical analysis resolves organized beings into organs and tissues and cells and physiological units, but does this analysis give an adequate account of organization?

The bodies of two allied animals are alike in structure. They are composed of organs which are said to exhibit fundamental unity *behind* superficial diversity, for they are practically identical in history, and for most of the purposes of the anatomist and the physiologist and the zoologist. From this point of view, and from many others, they are identical in structure, yet the differences between them do not cease to be because they do not concern us, nor because they escape our notice, for while the identity is real and important and significant, it has no abstract, or independent, reality.

'Were the heart of one man,' says Maudsley, 'to be placed in the body of another, it would probably make no difference in the circulation of the blood, but it might make a real difference in the temper of his mind.' Does not the analogy of nature lead us to ask whether it might not be expected to make a difference in the circulation of his blood as well as in the temper of his mind? If our knowledge of hearts were as minute and individual as our knowledge of men, might we not need a proper name for each heart as much as we need one for each man?

If the interest of the histologist in the resemblances between the tissues of one animal and those of another leads him to lose sight of their constitutional differences, he is in danger of mistaking an abstraction for a reality, for while the scientific basis of histology in the resemblances between the tissues of one animal and those of another is real and significant, it has no abstract, or independent, reality.

"From the morphological standpoint," says Hertwig, quoting from de Vries, "we may properly regard the cell, apart from the organism, as an individual, but we must not forget that it is by abstraction that we do so. Physiologically the cell is an individual only when actually isolated and independent of an organism. From this standpoint, every abstraction is a blunder."

When we say a multicellular organism is a unit, must we not also say what are the relations with reference to which it is a unit? When we say its constituent cells are units, must we not also say what are the conditions with reference to which they are units? Have we any way to find these things out except scientific discovery?

18. *Is cell-differentiation inherent or induced?*

A thoughtful and distinguished naturalist tells us that while the differentiation of

the cells which arise from the egg is sometimes inherent in the egg, and sometimes induced by the conditions of development, it is more commonly mixed; but may it not be the mind of the embryologist, and not the natural world, that is mixed? Science does not deal in compromises, but in discoveries. When we say the development of the egg is inherent, must we not also say what are the relations with reference to which it is inherent? When we say it is induced, must we not also say what are the relations with reference to which it is induced? Is there any way to find this out except scientific discovery?

19. *Are the beneficial effects of practice and training and education and opportunity innate or superadded?*

Can we hope to answer this question, *a priori*, by deduction from hypotheses? Is there any more value in Weismann's demonstration that acquired characters cannot be inherited than there is in Haeckel's declaration that the inheritance of acquired characters is a necessary axiom of the monistic creed?

Such facts as are in my possession seem to me to show that, while we need opportunities to make the best of our natural abilities, no one can do his part in any station in life without natural aptitude. As my opinion is not a deduction from a hypothesis, I hold it lightly, and subject to revision and correction.

20. *May not the biological notion of a living substance be an illustration of the fallacy of the undistributed middle?*

When we say all living things are alike in substance, I cannot discover that we mean anything more than we mean when, admitting some report of a conversation as a substitute for the truth for some purpose that we have in view, we say it is the same in substance as the original conversation.

The modern naturalist is so well aware of the endless diversity of living things that he never—that is, hardly ever—thinks that because one amœba, or one yeast-plant, or one horse, will serve certain purposes of experiment, and demonstration, and instruction, as well as another, they are alike in any respect whatever.

21. *Conclusion.*

As my only purpose is to do what I can to make the way clear for the progress of embryological science, by trying to free my own mind, and the minds of others, from all notions which imply that embryological science is impossible, and not to give a natural history of mind, I have passed by many important aspects of human knowledge without notice. But, before I close, I ask you to take away with you, and to consider, this familiar fact: Philosophers tell us we may come at truth by deducing it from certain first principles which are self-evident to the normal man, and they talk about the normal man as if he were a prominent citizen, the familiar acquaintance of all who have any claim to be considered men of intellect, and a well-known face even to the common herd. The naturalist declares he knows no such person, that all men are individual and particular men, and the normal man a fictitious character, and a statistical average without opinions.

If the naturalist is honest with himself, it seems to me that he cannot fail to come in time to hold his most cherished convictions subject to revision, and to value them only when they are verified by laying them alongside nature, and to regard absolute truth and necessary truth as meaningless words, because the being of things is not absolute but relative to everything else in nature.

The truth that knowledge is not absolute, but relative, is held to be the final and conclusive proof that we can never know anything as it really is, for we are

told that the reality behind the phenomena of sense must be unknown and unknowable, because we can never come at absolute truth. But may not the naturalist be moved to ask whether the conclusion follows from the premises? May it not prove to be only the final transformation of the protean fallacy of the undistributed middle? Instead of showing that we can never know anything as it really is, may not the relativity of knowledge show that nature, as it really is, is relative and dependent—that its being is not in itself? "As no man fording a swift stream," says Huxley, putting into vigorous English a thought that has often found expression; "as no man fording a swift stream can dip his foot twice in the same water, so no man can, with exactness, affirm of anything in the sensible world that it is. As he utters the words, nay, as he thinks them, the predicate ceases to be applicable; the present has become the past; the 'is' should be 'was,' and the more we learn of the nature of things, the more evident is it that what we call rest is only unperceived activity. Thus the most obvious attribute of the cosmos is its impermanence. It assumes the aspect not so much of a permanent entity as of a changeful process, in which naught endures save the flow of energy and the rational order which pervades it."

Every reflective student will, no doubt, feel a responsive chord vibrating in his own thoughts in unison with those of Huxley; but should he not ask himself whether the words, '*flow of energy and the rational order which pervades it,*' mean anything, except that the reality in which the flowing river of nature endures and has its being is rational energy, the energy of a reason, the activity of a mind?

Biological science seems to me to show, with ever-increasing emphasis, that it is in one sustaining mind that we ourselves, and all we know, or can hope to know, have being. Even if this be neither absolute truth nor necessary truth, may it not be that still better truth, a scientific discovery; and the greatest of all scientific discoveries because it has, so far, been verified in every act of knowing?

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THE NATURE OF NERVE STIMULATION AND OF CHANGES IN IRRITABILITY.*

As the conclusions of this paper supplement those of Professor Loeb, and as he is unable at present to publish an account of his work simultaneously with mine, a brief statement of the relationship of our work appears to us both to be desirable.

It is well known that Professor Loeb has for the past several years been applying the conclusions of physical chemistry in the investigation of the phenomena of life, as he was convinced that these conclusions would clear up many physiological phenomena. Of the several discoveries which have rewarded his insight there are two of apparently the most fundamental nature. One of these was made several years ago and published in *Fick's Festschrift* in 1899. It consisted in the demonstration that muscle would only beat rhythmically in solutions of electrolytes. This practically established the fact that contractility was in its essence an electrical phenomenon. About two years ago he expressed to me the opinion that other life phenomena were electrical, and not chemical or thermodynamical. A second fundamental generalization was made last summer at Woods Holl and published in *Pflüger's Archiv,* Volume 88, 1901, to the effect that the toxic and antitoxic action of salts was a function of the number and sign of the elec-

* This paper was prepared for publication early in January, but has been delayed in its appearance.

trical charges their ions bore. During the summer, and later in a lecture before the Medical Society of the University of Chicago, he applied these results to the action of toxines and antitoxines. He was, however, unable to discover any series of facts for the anions similar to those he established for the kations, and he referred the poisonous action of a pure sodium chloride solution to the monovalent kations the salt possessed instead of to the anions. In his work on muscle, also, the stimulating action of sodium chloride was referred to the sodium ions and I provisionally adopted the same explanation in my preliminary paper on the action of salts on nerves published in the *Journal of the Boston Society of Medical Sciences* last spring. Professor Loeb's attention was thus drawn chiefly to the kations. He attributed the undoubtedly greater stimulating action of the bivalent and trivalent anion sodium salts to their calcium-precipitating properties, having been brought to this conclusion by the peculiar action of fluorine.

In 1897 Professor Loeb directed my attention to physical chemistry, and his results on muscle appeared so remarkable that I began, three years ago, a study of the stimulating action of salts on nerves. The relationships were so complex that a long series of observations were necessary, but, during the spring of 1901, I published a preliminary paper in which, owing to incomplete results, I fell into several errors. The stimulating action of the higher anions was provisionally referred to the hydroxyl ions the solutions generally contained, and the peculiar activity of sodium compounds to the diffusion of Na ions into the muscle.

Further experiments showed me that certain of the conclusions were wrong. After reading Hardy's paper on 'Colloidal Solutions' and hearing Professor Loeb's lecture on the possible importance of the valence of ions, and more particularly of the kations, in determining their poisonous character, I had the opportunity of putting together my experiments. After computing the degree of hydrolysis and the number of H and OH ions in the solutions, it appeared that it was not the OH ions which were the cause of the stimulating action of the borates and citrates. The resemblance of my results to those of Hardy on colloidal solutions was apparent, and, apart from certain exceptions left for future investigation, I was led to infer that stimulation was due to the negative ions, and that the positive ions prevented stimulation; and also that as the stimulating action of the anions generally increased with an increase in valency, the stimulation was due to the electrical charges the ions bore. Following out this idea, which, as will be seen, was the extension of Loeb's idea of the importance of valence, the electrical relationships of nerves, the nature of stimulation and of changes in irritability and the nature of the nerve impulse appeared in a new light. The main results and conclusions were presented before the Medical Society of this University on December 2.

Meanwhile, unknown to me, Professor Loeb had begun to doubt that the calcium precipitation by the higher anions was the real cause of their action. Upon hearing my results and conclusions he perceived that they agreed with his facts as well.* It is with gratitude that I acknowledge my indebtedness to Professor Loeb, who showed me in which direction to look and who as a pioneer has opened one of the most fruitful fields of science. My own conclusions supplement and, it seems to me, make more precise those general ideas which were guiding him.

* Since this paper was sent to the editor, Professor Loeb has published in the February number of the *American Journal of Physiology* a portion of his results on muscle.

My observations were made on the sciatic nerve of the frog and stimulation of the nerve was shown by the contractions of the gastrocnemius muscle. I have tried about nine hundred experiments on frogs at different seasons of the year, so that the observations are numerous enough to offset most individual variations. The nerves were immersed for the greater part of their length in the solutions to be tested.

1. Nerves are stimulated by the withdrawal of water. The non-electrolytes sugar, urea and glycerine will stimulate if the osmotic pressure of their solutions is equal to, or greater than, twelve atmospheres. This is more than twice the osmotic pressure of the nerve. Nearly all electrolytes tested, quite irrespective of their nature, will also stimulate if their solutions are as concentrated as this. The nerve always increases in irritability (katelectrotonus) before impulses large enough to cause the muscle to contract are generated. After complete loss of irritability in the non-electrolytes the nerves will be completely restored by placing them in M/8 sodium chloride solution. These facts demonstrate anew the truth of the generally accepted opinion of physiologists that the change in the nerve which generates the nerve impulse can be set up by the withdrawal of water.

2. All salts of H, Li, K and NH_4 which were tested of which the anions are monovalent, such as KCl, KBr, KI, LiCl, NH_4Cl, and others; all salts of bivalent kations united to monovalent or bivalent anions, such as $MgCl_2$, $MgSO_4$, $Mg(NO_3)_2$, $ZnCl_2$, $ZnSO_4$, $BaCl_2$, $Ba(NO_3)_2$, $CuSO_4$, $SrCl_2$; all salts of trivalent kations united to monovalent anions, such as Fe_2Cl_6 and Al_2Cl_6 will generally stimulate if their solutions have an osmotic pressure of twelve atmospheres or over. In solutions weaker than this they all annihilate nerve irritability without stimulation, H, K and Fe_3 salts most rapidly. Irritability may generally be restored, if the nerves are not left too long in the solutions, by immersion in M/8 NaCl solutions. All these salts, therefore, stimulate by withdrawing water. The salts themselves will in each case destroy irritability.

3. All acids tested with the exceptions (possibly) of phosphoric and oxalic will not stimulate, except in solutions of high osmotic pressure (twelve atmospheres) Hydrogen ions do not appear hence to stimulate the nerve. On the contrary in weaker solutions tested, nerve irritability was lost without stimulation. This confirms Grützner and others. My experiments, however, are not complete on this point.

4. Alkalies such as NaOH, LiOH, KOH, $Ba(OH)_2$ will stimulate in approximately N/20 solutions. The hydroxyl ion, in other words, at certain concentrations stimulates the nerve.

5. If we compare the stimulating action of NaCl, NaBr, and NaI we find that these salts stimulate even in solutions of the same osmotic pressure as the nerve. The stimulating action of the salts increases as we pass from the chloride to the iodide. Hence stimulation is in some way a function of the anion, because the rate of diffusion of these salts is approximately the same and the number of Na ions is constant. It is not a function of the atomic weight, since the fluoride stimulates more than the chloride or iodide. These observations confirming Grützner led to the conclusion that the stimulating action of salts is due to their anions. On comparing the action of Na_2SO_4, $Na_2C_2O_4$, Na_2HAsO_4 and other bivalent anion salts we find that these are more powerful than the monovalent anions; and the trivalent anion salts such as sodium ferricyanide, sodium citrate and Na_3PO_4 are still more powerful than the bivalent anion salts. Thus NaCl and NaBr

will stimulate slowly in solutions of one gram molecule to 8,000 c.c.; Na_2SO_4 in one gram molecule to 25,000 c.c.; and Na_3 citrate in solutions of one gram molecule to 50,000 c.c. The power of stimulation as indicated by the prolonged tetanic and simple contractions of the muscle extending over hours is also greater than that of the monovalent salts. These observations clearly support the inference that stimulation is a function of the anions and also establish the fact that it is a function of the charges the ions bear. They thus support Loeb's general idea that valence or the electrical charges of ions determine their physiological action, but demonstrate that it is the negative ions which stimulate. As will presently be shown, however, valence, as such, possibly has no direct influence, but only indirectly determines the action of these ions.

6. The conclusions just drawn led me to infer that the positive ions must prevent stimulation and render the nerve non-irritable. This is shown to be the case by a comparison of HCl, LiCl, KCl, NH_4Cl and NaCl. The last salt stimulates; in the others the chlorine ion will not stimulate and the nerves lose their irritability. This can only be explained, I believe, by assuming that the stimulating action of the chlorine ion is overbalanced by the non-stimulating action of the positive ion, and of these positive ions it appears that H overbalances most, K less, Li still less and NH_4 least. If this idea is true it should be possible, by combining these positive ions with di- and trivalent more potent anions, to obtain a stimulating compound. This is indeed the case. KCl never stimulates except by the withdrawal of water; K_2SO_4 will occasionally stimulate the most irritable nerves in solutions of about the osmotic pressure of the nerve; K_3 citrate and K_3 ferricyanide will stimulate in solutions of a gram molecule to 22,000 c.c., of which the osmotic pressure is less than that of the nerve. The same is true for other salts. Li_3 citrate stimulates in a gram molecule to 30,000 c.c. and $(NH_4)_3$ citrate in a gram molecule to 40,000 c.c. We thus come to the conclusion that stimulation is due to the negative charge of the anions and that the kations prevent stimulation. It follows from this that the chemical properties of an acid or a salt are determined by the balance between the anion and the kation. In NaCl the ions are nearly equivalent, but the chlorine slightly overbalances. This idea of the mutual antagonism of the anion and kation may possibly throw light on chemical processes and properties generally.

7. $KMnO_4$, $NaMnO_4$, and $NaClO_3$ will stimulate in solutions of a gram molecule to 12,000 c.c. This stimulation is possibly due to the liberation of some bivalent oxygen anions.

8. These results are similar to those of Hardy and others on colloidal solutions. Colloidal solutions, the particles of which are positively charged are precipitated by OH ions and anions and the precipitating action of these anions is in proportion to a power of their valence. They seem to be held in solution by hydrogen and possibly other positive ions. As it is well known that protoplasm contains colloids in solution, a fact Hardy has particularly emphasized, it occurred to me that stimulation might be due either to a gelation of the colloids or to their passage into solution. Loeb has frequently mentioned his belief that a variation in the state of the colloids in protoplasm is of importance in protoplasmic activity and particularly irritability. He and others have repeatedly described processes of liquefaction of protoplasm, and several years ago he attempted to refer changes in irritability to an alteration in the viscosity of protoplasm. I infer that stimulation consists in a

passage of the particles from the solution to or toward the gel, and that if we can prevent gelation stimulation is prevented and irritability is lost. This is indicated by the following facts among others:

The nerve contains colloids. Colloidal solutions, the particles of which carry positive charges, are precipitated by negative ions. Nerve irritability is increased by cooling and diminished by warming. The stability of the hydrosol is probably diminished by cold and increased, like common gelatine, by moderate warmth. Also when coagulation by heat occurs the nerve is stimulated. Coagulation is but the formation of an irreversible gel. Darwin's observations on *Drosera* and other plants by optical evidence demonstrates also this gelation. Darwin observed in his work on 'Insectivorous Plants' that the passage of the impulse over plant cells, which corresponds to the nerve impulse in animals, was accompanied by a visible precipitation or gelation of the protoplasm, the nature of which he did not understand, but which he called aggregation. He states that the molecular change supposed to occur in nerves may thus actually be seen in plant cells. There can be no doubt that he was right in comparing this change to the nerve impulse. He found that it was produced most readily by the citrates and phosphates, and was checked by Ba, K and other such salts. Thus his facts correspond closely with those I have found for the nerve. Aggregation was prevented by ether, by CO_2 or lack of oxygen. It could be produced by the extraction of water. His description of the process leaves little doubt that he is describing the formation of a reversible gel. The aggregated particles afterwards dissolved. The action of anæsthetics and the electrical phenomena of the nerve also support the idea that stimulation is a process of gelation. This will be discussed later.

These facts indicate the truth of the following general statements:

I. Protoplasm consists essentially of a colloidal solution, the particles of which are positively charged. It is a reversible hydrosol.

II. Stimulation consists in the passing of the solution to or toward the gel. Irritability is reduced or abolished if we make the sol state more stable, or if gelation is complete. In other words, irritability varies inversely with the stability of the hydrosol.

9. Electrical stimulation. If stimulation is due, as I believe, to the negative charges the ions bear and is prevented by the positive, the identity of electrical and chemical stimulation is thus demonstrated. It makes no difference whether we put the negative charges into the nerve on ions or whether by touching the nerve with electrodes we bring about, so to speak, a surplus of positive charges at one pole and negative at the other. The end result is the same. It is thus plain why the stimulus begins at the negative electrode, or kathode. In this region by the action of the kathode, the negativity of the nerve is increased and gelation occurs. In what manner this negativity is increased will be discussed in the full paper, but it may be due indirectly to the hydroxyl ions. Electrotonus is also explained. By the passage of the current the negative charges are in excess or preponderate in their action near the kathode and positive charges preponderate near the anode. The stability of the hydrosol is diminished near the former and increased near the latter. Irritability is altered as just explained. These conclusions are supported by Hardy's observations on the movement of colloidal particles in the electric current and their precipitation at the kathode if positively charged. If the nerve is already near gelation (very irritable by cold or drying) we may have

gelation occurring sufficiently abruptly to cause tetanus during the passage of the current. A true condition of katelectrotonus may also be produced by taking water from the nerve.

10. The current of injury may also be explained on this hypothesis. At the cut end aggregation or gelation is taking place as a result of the disturbance of the mechanical conditions of the nerve. Here positively charged colloids are going out of solution and negative charges are temporarily set free. The cut end becomes negative to the uninjured portion. This conclusion is supported by the fact that warming the nerve locally causes the warmed portion to become electro-positive to the unwarmed, and cooling it causes the cooled portion to be electro-negative to the rest of the nerve. It is similar to what occurs when zinc goes into solution. The undissolved zinc becomes negative to the solution. It is also well known that if by heat we produce an irreversible gel (artificial heat section) of the nerve, the coagulated portion is negative to the rest.*

11. Mechanical stimulation may possibly be understood as follows: By the mechanical coalescence of the neighboring colloidal particles their surfaces become less than the sum of the surfaces of the separated particles. A portion of the negative charges formerly induced in the water surrounding each particle is accordingly set free. These immediately act like negatively charged ions and precipitate the next layer of colloids. The process may possibly be similar to that which occurs on jarring an unstable hydrosol or a supersaturated solution. The observations of Darwin and others show that jarring will bring about aggregation in protoplasm.

12. The nerve impulse may consist in the following process. By the precipitation of each layer of colloids negative charges are regenerated; these precipitate the next layer of colloids and are again regenerated, and so on. That something of this sort occurs is indicated by the following facts:

(*a*) Darwin's observation that the passing of the impulse in plant cells is accompanied by a progressive precipitation.

(*b*) The facts that negative charges are set free in the nerve by the action of each successive segment. These charges constitute the negative variation.

(*c*) The fact that negative charges precipitate positively charged colloids.

(*d*) The fact that negative charges stimulate the nerve.

(*e*) The fact shown by the action of ether and other poisons that the negative variation is not a simple movement of inorganic ions, but is dependent for its propagation upon the state of irritability (state of the colloids) of the nerve. This fact has already led many physiologists to infer that the negative variation stimulates each successive segment of the nerve and is regenerated by the change it itself has brought about.

13. The action of anæsthetics. This consists, on the hypothesis so far sketched, in increasing the stability of the hydrosol or solution, and so preventing precipitation. There can be no doubt that the anæsthetics have this action as is shown by the following facts: Darwin observed that they prevent the process of aggregation or precipitation in plant cells, and it has been shown by Loeb, Budgett, Zoethout and others that they liquefy or dissolve the cells of infusoria and other animals and egg cells. The effect of a mixture of ether and water on starfish eggs is remarkable. The egg dissolves in it very rapidly. Furthermore, Overton and Meyer have shown that the anæsthetizing action of substances is pro-

* The relation of this explanation to Waller's idea that the cut end is positive will be discussed in the full paper.

portional to their fat-dissolving powers. The colloids in protoplasm are in all likelihood fat or lecithin proteid combinations like the sheath of the red blood corpuscles, and like the latter they are, no doubt, more soluble in ether and water than in water alone. So far as I can see, this explanation of the action of anæsthetics is in harmony with the facts. It supports the general conclusions drawn as to the meaning of changes in irritability and it explains the often-noted similarity of action between hydrogen ions, certain poisons, potassium ions and the anæsthetics. All these substances increase the stability of the hydrosol and liquefy protoplasm.

14. Stimulation by light and ether vibrations. In paragraph 5 it was stated that, in my opinion, stimulation by the negative ion was not due primarily to the valence of the ion. It is not the charge itself, but its motion, which determines stimulation. This is shown, I believe, by the variation between the action of fluorine, chlorine, bromine and iodine, and between potassium, sodium and hydrogen. The hydroxyl ion, although monovalent, stimulates like a bivalent anion. Since the fact is apparently established that it is the electrical charge which stimulates, and not the atom with which it is associated, and also since the charge associated with chlorine does not differ in nature from that associated with fluorine, the difference in action between these ions can only be due to something the charge does; in other words, to the motion of the charge or of the atom with which it is associated. When a charge is moved it produces a disturbance in the ether. It is well known to all that the vibrations of the ether will produce those changes in protoplasm which the ions produce, and further the character of the change in protoplasm produced by light varies with the wave-length or the number of impacts per second. Violet light or the ultra-violet rays stimulate protoplasm, while the red rays as a rule do so very feebly or inhibit movement. By the electromagnetic theory of light the ether disturbances which we call light must be due to the movement of electrons or charges in the sun, either constituting a part of the sun's atoms or associated with these atoms. In other words, it is not the presence of the charges in the sun which stimulates protoplasm, but the movements of the charges.

These facts are ground enough for the hypothesis that it is not the charges or the number of charges, but the movements of the charge which produce the change in protoplasm called stimulation, and, I may add, which must determine chemical action as well. This idea will agree, I believe, with the suggestions of J. J. Thomson, Larmor, Nernst and others in regard to the association between atoms and electrons. This motion of the electron may be either translatory on the atom, which will agree with the kinetic theory of solutions, or it may be a rotatory motion. For various reasons I am inclined to assume that the charge is either revolving with the atom or about it, but a detailed consideration of this point will be given in the full paper. Knowing, however, that charges in motion affect the ether; that the impulses thus given produce chemical changes; that substances in solution or as solids actually give out what we call ether vibrations; having established the fact that monovalent ions differ among themselves in stimulating action, although the charges are the same on each, and also that ions stimulate by the charges and not by the atoms, I see no escape from the conclusion that it is not the charge, but its motion and its sign, which ultimately determines its action. In other words, chemical stimulation and light stimulation are identical.

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NODULES AND MOLECULES OF RED BLOOD-CORPUSCLES.

DESCRIPTIONS of human blood usually give the general form and dimensions and behavior of the red corpuscles, emphasizing the fact that mammalian blood in the mature condition is negatively characterized by the absence of nuclei. They fail, however, to note the presence of minute nodules, like excessively minute nuclei, one for each corpuscle as a general rule. These bodies, which are not always central in the corpuscle, appear under the microscope as dark rings, each with a bright, yellowish center. On first seeing them in coagulated human blood, I was puzzled by their being unexpected. Afterwards I found a row of them visible in profile along the edge of a layer of blood that had got bent up. In this case they were like minute mammæ, spherules protruding so as to show the yellow hue without a dark border. They soon came to be the best evidence of the presence of blood, being seen under the microscope at regular distances, as marking the component corpuscles of the clot; and they persist as the last recognizable parts of disintegrating blood.

Not being able to find any reference to them in our own language, I was directed by my colleague, Professor C. F. W. McClure, to an article on them by A. Negri in the *Anatomischer Anzeiger* of 1899, p. 33. That article referred to their discovery and description by Petrone of Catania in 1897; and reported an examination of human and dog's blood, comparing the nucleated condition of the red corpuscles of the fœtal blood with the non-nucleated condition in the adult. And after describing the form, aspect and position in the mature blood, of the bodies to which we may assign the name 'blood-nodules,' it described and figured a small body attached to the nucleus in the fœtal blood; adding that this is the body which after decay of the nucleus itself in mammals persists in the adult, and that it is not found in non-mammals.

After studying the account of Hæmoglobin, by Gamgee in the first volume of Schaefer's 'Text-book of Physiology,' I attempted to apply the results of the chemical work and the spectroscopic examination by recent authors to the problem of the molecular constitution of the blood corpuscle. According to Hüffner and others the hæmoglobin molecule is, chemically speaking, very large, numbering 16,669 as its molecular equivalent; and the explanation of this largeness is that it carries one atom of iron, which, being itself heavy, 56, requires a large vehicle, just as a gunboat is large because it is to carry a heavy cannon. The final cause of this arrangement appears to be that the molecule of hæmoglobin may insorb a molecule of oxygen gas, becoming specially associated with its atom of iron, in the form of FeO_2, receiving the charge of oxygen at the lungs, and afterwards discharging it into the tissues. This suggested the possibility of determining in an approximate way the absolute size of the molecule of hæmoglobin. I understand that this has not hitherto been done for any proteid; and the method here employed is general, and may be used wherever an organic substance combines in definite proportions with a gas.

Having measured the volume of the red blood-corpuscle, and taking 31 per cent. as its quantum of hæmoglobin, and 1.29 as the specific gravity (estimated from the whole corpuscle being about 1.09 sp. gr., of which 69 per cent. is water), I made out in milligrams the weight of the hæmoglobin for one corpuscle. Applying to this the well-established constant that one milligram of hæmoglobin insorbs 1.334 cubic millimeters of oxygen gas estimated at 0°C. and 760 mm. pressure, the product of these gave the volume of oxygen gas in-

sorbed by the single corpuscle as its full charge. Nernst's 'Theoretische Chemie' (1900, p. 394) gives the most reliable estimate of the number of molecules of oxygen, or any other gas, in a cubic millimeter at standard temperature and pressure. This is 55 thousand millions of millions (which may be written 55TMM). Calculated from this, the oxygen taken in by the single blood corpuscle as a full charge is found to be about 28 hundred millions of molecules. But as the combination is known to be regularly one molecule of the gas to one molecule of hæmoglobin, this result, or in round numbers three thousand millions, is approximately the number of hæmoglobin molecules in the blood-corpuscle (3 TM).

Dividing this last number into the volume of the hæmoglobin in a corpuscle, we obtain the volume of the cubic 'room' assigned by chemists to each molecule, and the cube root of this will give the length of the imaginary walls of said room, also nearly the diameter of the molecule regarded as a sphere in a solid state. The volume is approximately $1/10^{17}$ cubic millimeters, and the linear dimension of the side of a molecule 'room' is about 1/500,000 of a millimeter. The 'rooms' of the oxygen molecules in the gaseous condition are much larger than these, because the gases rejoice in spacious apartments; in fact, the volume of gas which is insorbed by the blood is nearly twice as great as that of the devouring hæmoglobin.

Nernst states that by multiplying the absolute atomic weight of hydrogen upon the molecular formula of any proteid, we may obtain the absolute weight of the proteid. This involves, we think, the assumption that no condensation has occurred in building up proteid molecules. In order to test the rule by hæmoglobin, we find that this rule gives as the absolute molecular weight $1.35 \times (10)^{-17}$ of a milligram. By the method of the quantitative absorption given above of oxygen the value comes out as $1.30 \times (10)^{-17}$ of a milligram. The two results differ by less than 4 per cent. This close harmony does not prove that the estimated weight of the atom of hydrogen is right, for it enters into both methods; but it does prove non-condensation, and also confirms the quantitative results of Hüffner and others as to the absorption of oxygen. It may be added that the oxygen absorbed is, when estimated in its fluid form, about 1/470 the volume of the absorbing hæmoglobin.

But probably if the oxygen were examined in the liquefied or solidified condition, its molecular sphere of action would be found not to be so very widely divergent from its rightful proportion of 32 to 16,669.

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SCIENTIFIC BOOKS.

Leçons sur les séries divergentes. Par ÉMILE BOREL, maître de conférences a l' Ecole Normale Supérieure. Paris, Gauthier-Villars. 1901. Pp. vi + 182.

La Série de Taylor et son prolongement analytique. Par JACQUES HADAMARD. Scientia, série physico-mathématique. Chartes, imprimerie Durand. 1901. Pp. viii + 102.

These two works can appropriately be classed together, on account of both their authorship and their contents. Among the younger French mathematicians who have taken their doctors' degrees within the past dozen years none are to-day more conspicuous than Hadamard and Borel. Their theses were published in 1892 and 1894 respectively. A few years later both writers were recipients of prizes from the French Academy of Sciences. In 1896 Hadamard received the 'Prix Bordin' for his work on geodesics, while Borel won the 'Grand Prix des sciences mathématiques' in 1898 for his investigations upon divergent series. Recently also they have been bracketed in a list of nominees to fill a vacancy in the Academy of Sciences.

We have here to consider two representative

works. Each has the special interest that it is devoted to that branch of the theory of functions in which its author first attained distinction. Hadamard aims to give a concise, almost an encyclopedic, résumé of the present state of our knowledge concerning the analytic continuation of Taylor's series. Borel, on the other hand, gives a more detailed exposition of a single chapter of this subject, the divergent series. On this account his book will have the greater interest for the mathematical public and will be reviewed at somewhat greater length.

Two other works of equal size and somewhat similar character have been previously published by Borel, his 'Leçons sur la théorie des fonctions' (treating the 'Eléments de la theorie des ensembles et applications') and his 'Leçons sur les fonctions entières.' Together with the present work they form a unique series, embodying the results of much recent investigation in the theory of functions. It is indeed a piece of rare good fortune in any province of mathematics to have the important recent work thus promptly picked out and thrown into accessible form by such a mathematician as Borel. For this reason the publication of these lectures cannot be too warmly welcomed.

It is safe to say that no previous book upon divergent series has ever been written. Borel opens up a field of research which is still very new and promises rich reward to the investigator. In the process of evolution the divergent series has passed through several curious stages of development. At first a divergent series was accepted on faith and used with great *naïveté*. Thus Leibnitz, for example, when considering the expansion of $1/(1+x)$ into the series $1-x+x^2-x^3+\ldots.$ remarks that if $x=1$, the sum of n terms takes alternately the values 1 and 0, and the sum of the series must therefore be equal to the mean value $\frac{1}{2}$. After the introduction of exact analysis by Cauchy and Weierstrass such a loose mode of treatment could no longer be tolerated. The mass of inconsistencies to which it would lead was clearly perceived, and a divergent series was therefore considered by the mathematician to be meaningless, good for nothing but to be thrown away. However, a few of the great mathematicians were visibly perturbed over the situation. Thus we find Cauchy complaining in 1821:

"J'ai été forcé d'amettre diverses propositions qui paraîtront peut-être *un peu dures;* par exemple, qu'une série divergente n'a pas de somme."

We know also that Abel was only prevented by his premature death from attacking the problem. But the view that a divergent series had no place in mathematical analysis soon became orthodox, and search after a legitimate basis for its use was abandoned. Nevertheless the astronomers, in utter disregard of this opinion, still continued to employ divergent series and to obtain from them a sufficient degree of approximation for practical purposes.

The impetus to a new mathematical treatment of the subject may be said to have come simultaneously from Stieltjes and Poincaré, although prior to this, in 1880, the legitimacy of the conclusion of Leibnitz had been established by Frobenius in a memoir which was suggestive of the beautiful theory developed later by Borel. According to the new view a divergent power-series is considered as having value in two distinct ways, either as enabling one to find an approximate value of some corresponding function (Poincaré and Stieltjes) or as a source of another algorithm which is convergent and therefore defines a proper function (Stieltjes, 1894).

The treatise of Borel begins with an interesting historical introduction. The body of the book can be divided roughly into four parts, which take up successively the four chief theories of divergent series; the asymptotic theory of Poincaré, the continued fraction theory of Stieltjes, the theory of Borel—characterized by the use of definite integrals containing a parameter z—, and, finally, the theory of Mittag-Leffler. The crowning achievement is without doubt Borel's own work, and his presentation of it is the most interesting feature of the book. No adequate idea, however, of the treatment of Stieltjes can be obtained without direct reference to the famous memoir of 1894, as Borel frankly

states. This inadequacy of presentation is offset by the addition of an important supplement which Borel himself contributes to the theory of Stieltjes. We regret the omission of the method of Lindelöf. Its dismissal with a half dozen lines and without even a reference to his article in the *Acta Societatis Fennicae* is possibly due to a certain haste in preparation which we have fancied we have detected in several places. While the method of conformal representation (or transformation of the variable) which Lindelöf employs has been applied only to a restricted class of divergent series, it seems probable that it could be developed so as to give a more general theory.

On account of its somewhat abstract character Borel's treatise will probably be of greater interest to the pure mathematician than to the astronomer or student of applied mathematics. Few applications of the various theories have been given, probably because but few applications have yet been made, except in the case of the asymptotic theory of Poincaré. The author leaves us in some uncertainty as to how far his own theory has been carried and applied to differential equations. We hope that in a subsequent edition the important applications will be more fully developed.

We turn now to the little book of Hadamard. This is one of a series of short monographs published under the general title 'Scientia' and devoted to the 'Exposé et développement des questions scientifiques à l'ordre du jour.' The special topic taken up by Hadamard, as has already been stated, is the analytic continuation of a power-series, $a_0+a_1z+a_2z^2+\dots$. In the consideration of this question two problems of the greatest importance and difficulty present themselves. These are: (1) The determination of the nature and position of the singular points of the analytic function defined by the series, and (2) the calculation of the value of the function at points exterior to the circle of convergence.

Hadamard has had the extremely difficult task of compressing into a few pages what has been done on these problems. In this he has succeeded admirably. It is extraordinary what an amount of information is packed away in the space of one hundred pages. Yet the work is no dry compilation of facts. Nowhere is the skill of the author more fully shown than in the manner in which he has woven his materials together. The theorems are analyzed, their significance is pointed out, and their demonstrations are outlined sufficiently to show the manner in which the subject is treated. Attention should also be called to the excellent bibliography with which the book opens and to which reference is constantly made. In correlating the one hundred and fifty memoirs here included Hadamard has performed a very important service. His admirable report is not suited to the reader who has little acquaintance with the general subject, but to the specialist and investigator it will be invaluable.

E. B. Van Vleck.

The Elementary Principles of Chemistry. By A. V. E. Young, Professor of Chemistry in Northwestern University. New York, D. Appleton & Company. 1901.

This book differs so radically from those in general use that if reviewed at all, it must be at some length. The author has used this method successfully for thirteen years; his object being to instruct the student during the first year by this method, which he calls the quantitative method. He says that its inception is due to Professor Josiah P. Cooke, of Harvard; he believes it 'both scientifically and pedagogically an improvement on prevailing methods.' The presentation of a topic in the text is to be studied by the student after performing the laboratory experiment illustrating the same.

The first 97 pages of the book are devoted to the physical and chemical properties of substances and to simple theoretic chemistry, including the fundamental quantitative laws of chemical action, the gas laws, atomic and molecular theory, kinetic theory of gases, structure and stereoisomerism. The author lays particular stress on the quantitative laws, and also on the laws of Gay Lussac, Dulong and Petit, Mitscherlich and Raoult, as illus-

trating the relation between equivalent weights and certain specific properties. The remainder of the book (147 pages) is 'On the relation between the properties of the elements in general and their combining weights; description of the first twenty-five elements and some of their compounds.' These elements are those comprised in the first three horizontal series of Mendeléeff's chart of the periodic system. The properties of the commoner elements of this selection and their compounds are described in considerable detail. Here the book proper ends. A second part (106 pages) gives the experimental illustrations and instruction in details of laboratory work. The book is illustrated by full-page portraits of many of the chemists and physicists mentioned.

This is indeed a different treatment from that commonly followed. A course in chemistry in which copper, mercury, silver and lead are ignored, while beryllium and cobalt find consideration is not common. Yet this does not prove that it is wrong. The author lays chief stress on general laws. The student's comprehension of a law is based on a roughly quantitative experiment illustrating it which he performs before studying the law. The experiments merit attention; they are well devised and easy to perform. The author illustrates these laws further by the behavior of a number of elements, including important metals, and most of the important acid-forming elements.

It is not the object of a college course in science to form specialists, and the question may be fairly asked whether the mental discipline and the capacity to pursue the study of chemistry afforded by this method are not of equal value, or (as the author believes) of greater value than can be obtained by the prevailing method. To those who agree with the author this book should be welcome.

The book has one grave defect, in omitting all mention of electrolytic dissociation. The author anticipates criticism in a passage on page eight of a pamphlet called 'Suggestions to Teachers' which accompanies the book; he says: 'Some perhaps would wish to include osmotic pressure and the electrical phenomena of conductivity, etc., together with the theory of ionization, but I have judged it impracticable to illustrate these phenomena experimentally without displacing other matter or going beyond the reasonable scope of one year's work.' To this the obvious answer is, that with our present knowledge it would be better to displace other matter, if need be, than to omit anything so fundamental and so easy of illustration as electrolytic dissociation, from a book called 'Elementary Principles of Chemistry.' With the hope that this gap may be filled in the next edition, the reviewer commends Professor Young's book to the attention of college and advanced high school teachers, who will find it suggestive.

E. RENOUF.

Studies in Evolution. By CHARLES EMERSON BEECHER. New York, Charles Scribner's Sons. 1901.

This is a notable volume. It is one of the series of the Bicentennial Publications of Yale University, and consists mainly of reprints of occasional papers selected from previous publications of the Laboratory of Invertebrate Paleontology, Peabody Museum. The most important are those on the structure and development of trilobites, and the 'Studies in the Development of the Brachiopoda.'

The aim of the first essay, 'On the Origin and Significance of Spines,' is an attempt, in the terms of ontogeny, phylogeny and chronology, to apply the general law of evolution to the spines of plants and animals. The discussion is a very interesting one, and we think Dr. Beecher satisfactorily shows from a great number of cases discovered by numerous observers that spines are a characteristic of the old age, both of the individual and of the type. In old age the organism, during the senescence of the type, 'blossoms out with a galaxy of spines, and with further decadence produces extravagant vagaries of spines, but in extreme senility comes the second childhood, with its simple growth and the last feeble infantile exhibit of vital power.'

We are inclined to think that the author is a little too hospitable to Wallace's notion that spines on desert plants may originate from

the attacks of snails and browsing cattle. Our observations in the North African area, from Morocco to Egypt, on the edge of the Sahara in southern Algeria, and in Palestine, lead us to fully endorse the view of the Rev. Dr. Henslow, that in desert areas where plants are especially spiny or thorny, there are few snails, and a general absence of cattle. Over a century ago Pallas, and afterwards L. Regnier, in a paper published in 1792 (II., p. 101) in the very rare *Journal d'Histoire Naturelle,* edited by Lamarck and others, attributed the spiny growth of desert plants to the dryness of the soil. His observations appear to have been entirely overlooked by modern writers. A second article (p. 354), written by De Ramatuelle, is thoroughgoing in its evolutional tone, barring perhaps the speculations as to the origin of the spines from 'germes particuliers.'

Professor Beecher's splendid discovery of the nature of the appendages of trilobites and of other important points in their anatomy has entitled him to the lasting gratitude both of paleontologists and zoologists. This reprint of his original papers and illustrations is very opportune. It is possible, however, that the last word has not been said as to the nature of the larval trilobites or as to the position of the trilobites in nature. How the protaspis stage of trilobites can be likened to the nauplius of crustacea, and why trilobites should be placed among crustacea, we do not understand. That the presence of antennæ necessarily obliges us to regard trilobites as crustacea, when all the succeeding appendages of the body are of the same general type, not being differentiated into specialized mandibles, maxillæ, maxillipedes, thoracic and abdominal legs, as they are in Crustacea, including the Phyllopoda (though in them the appendages of the trunk are alike), does not seem logical. We would prefer to regard the trilobites, merostomes and Arachnida as members of a phylum quite distinct from that of the Crustacea. Is it not probable that the rather artificial phylum of Arthropoda will eventually have to be divided into three phyla? The resemblances in trilobites to Crustacea seem to us to be a case of convergence. The papers on Brachiopoda are likewise of great interest and value, and are crowded with valuable suggestions. The line of thought is largely based on the work of the late Dr. A. Hyatt, whose philosophical and scholarly methods have had such a happy and fruitful influence on the new generation of paleontologists.

A. S. Packard.

SOCIETIES AND ACADEMIES.

THE AMERICAN PHILOSOPHICAL SOCIETY.

The scientific program of the general meeting to be held next week is as follows:

Thursday, April 3, 10:00 o'clock.

'The President's Address': Gen. Isaac J. Wistar.

'Origin of the Oligocene and Miocene Deposits of the Great Plains': Professor John B. Hatcher, of Pittsburg.

'The Upper Cretaceous and Lower Tertiary Section of Central Montana': Mr. Earl Douglass, of Princeton.

'Evolution and Distribution of the Proboscidea in America': Professor Henry F. Osborn, of New York.

'On South American Mammals': Professor William B. Scott, of Princeton.

'The Mammals of Pennsylvania and New Jersey': Mr. Samuel N. Rhoads, of Audubon, N. J.

'The Identity of the Whalebone Whales of the Western North Atlantic': Dr. Frederick W. True, of Washington.

Afternoon Session, 2:00 o'clock.

'On the Molluscan Fauna of the Patagonian Formation': Dr. H. von Ihering, of São Paulo, Brazil.

'A Comparison Between the Ancient and Recent Molluscan Fauna of New England': Professor Edward S. Morse, of Salem, Mass.

'Distribution of Fresh Water Decapods and its bearing upon Ancient Geography': Arnold E. Ortmann, Ph.D., of Princeton.

'Systematic Geography': Professor William Morris Davis, of Cambridge, Mass.

'On Drift Casks in the Arctic Ocean': Mr. Henry G. Bryant, of Philadelphia.

'The Isthmian Canals': Professor Lewis M. Haupt, of Philadelphia.

8:00 o'clock at the Free Museum of Science and Art.

'The Relation of the American University to Science': President HENRY S. PRITCHETT, of Boston.

'The Advancement of Knowledge by the Aid of the Carnegie Institution': President DANIEL C. GILMAN, of Baltimore.

Friday, April 4, 10:00 o'clock.

'Historical Investigation of the Supposed Changes in the Color of Sirius since the Epoch of the Greeks and Romans': T. J. J. SEE, Ph.D., of Washington.

'Recent Progress in the Lunar Theory': Professor ERNEST W. BROWN, F.R.S., of Haverford, Pa.

'On a New Method of Transiting Stars': Professor MONROE B. SNYDER, of Philadelphia.

'On the Evolution of Martian Topography': Mr. PERCIVAL LOWELL, of Flagstaff, Arizona.

'Results of Observation with the Zenith Telescope at the Sayre Observatory': Professor CHARLES L. DOOLITTLE, of Philadelphia.

'On the Spectra of Gases at High Temperature': Professor JOHN TROWBRIDGE, of Cambridge, Mass.

'On Some Equations pertaining to the Propagation of Heat in an Infinite Medium': Professor A. STANLEY MACKENZIE, of Bryn Mawr, Pa.

Afternoon Session, 2:00 o'clock.

'The Direction of Evolution in Color-Marks in Rock Pigeons': Professor CHARLES O. WHITMAN, of Chicago.

'On Biological Heredity and Organic Evolution': Professor GIUSEPPE SERGI, of Rome, Italy.

'Is Scientific Naturalism Fatalism?' A one-minute paper: Professor WILLIAM KEITH BROOKS, of Baltimore.

'On *Dichotoma*, a new genus of Hydroid Jelly-Fish': Professor WILLIAM KEITH BROOKS, of Baltimore.

'On the Continuity of Protoplasm': Professor HENRY KRAEMER, of Philadelphia.

'Further Experiments on the Physiological Action of Ions': Dr. JACQUES LOEB, of Chicago.

'The Embryology of a Brachiopod': Professor EDWIN GRANT CONKLIN, of Philadelphia.

'Relationship of the Gordiacea': Professor THOMAS H. MONTGOMERY, JR., of Philadelphia.

'The Spermatogenesis of *Oniscus Asellus* Linn., with especial reference to the history of the Chromatin': M. LOUISE NICHOLS, Ph.D., of Philadelphia.

Saturday, April 5, 10:00 o'clock.

'The International Catalogue of Scientific Literature': CYRUS ADLER, Ph.D., of Washington.

'A Classification of Economics': Professor LINDLEY MILLER KEASBEY, of Bryn Mawr, Pa.

'Experiments on Cytolysis': Professor SIMON FLEXNER, of Philadelphia.

'On Osteitis Deformans': Professor JAMES C. WILSON, of Philadelphia.

'The Influence of Acute Alcoholic Intoxication upon Certain Factors involved in the Phenomena of Hæmotolysis and Bacteriolysis': Professor A. C. ABBOTT, of Philadelphia.

'Blindness from Congenital Malformation of the Skull': CHARLES A. OLIVER, M.D., of Philadelphia.

'Race Elements in American Civilization' (illustrated by German Examples): Professor M. D. LEARNED, of Philadelphia.

THE AMERICAN ELECTRO-CHEMICAL SOCIETY.

ARRANGEMENTS for the first general meeting of the American Electro-chemical Society, to be held at Philadelphia on April 3, 4 and 5, are as follows:

Thursday afternoon, April 3, 2 P.M. Visits to places of interest.

Thursday evening, April 3, 8 P.M., at the Manufacturer's Club, inaugural meeting. This meeting will be devoted to the organization of the Society, adoption of a constitution and by-laws, election of officers, determining times and places of future meetings, discussing the question of publishing the transactions, etc.

Friday morning, April 4, 9 A.M., at the lecture hall of the John Harrison Laboratory of Chemistry of the University of Pennsylvania. Reading and discussion of the following papers:

'A University Course in Electro-chemistry': Professor JOSEPH W. RICHARDS, Ph.D., Lehigh University, Bethlehem, Pa.

'Electrodes': CLARENCE L. COLLINS, 2d, Niagara Falls, N. Y.

'Note on the Gladstone Tribe Couple': Professor WILDER D. BANCROFT, Ph.D., Cornell University, Ithaca, N. Y.

'The Nascent State': C. J. REED, Philadelphia, Pa.

'The Electrolytic Reduction of Lead': PEDRO G. SALOM, Ph.D., Philadelphia, Pa.

'Electrodeless Conduction in Electrolytes': CARL HERING, Philadelphia, Pa.

'On the Electrolysis of Sodium Nitrate and the Composition of the Developed Gases': C. W. VOLNEY, Ph.D., Keyport, N. J.

Professor CHAS. A. DOREMUS, M.D., Ph.D., Subject to be announced.

Friday afternoon, April 4, 2 P.M., at the John Harrison Laboratory.

'Current Electro-chemical Theories': Professor LOUIS KAHLENBERG, Ph.D., University of Wisconsin, Madison, Wis.

'A Zinc-Bromine Storage Battery': HERBERT H. DOW, Midland, Mich.

'Continuous Electrolysis of Solutions of Metals': N. S. KEITH, Ph.D., New York City.

'A Method of Electrolytic Production of Zinc from its Ores': SAMUEL S. SADTLER, Philadelphia, Pa.

'The Electrolytic Rectifier': Professor C. F. BURGESS, University of Wisconsin, and CARL HAMBUECHEN, Madison, Wis.

'On the Relative Speed of the Ions in Solutions of Silver Nitrate in Pyridine and Aceto-nitrile': HERMAN SCHLUNDT, Ph.D., Madison, Wis.

'Fall of Potential in Electrolytes': CARL HERING, Philadelphia, Pa.

'Caustic Alkalies and Chlorine by the Dry Electrolytic Process': CHAS. E. ACKER, Niagara Falls, N. Y.

Friday evening, April 4, after 8 P.M., at the Manufacturer's Club, 1409 Walnut Street. Informal reception.

Saturday morning, April 5, 9 A.M., at the John Harrison Laboratory. Reading and discussion of the following papers:

'On a New Type of Electrolytic Meter': KONRAD NORDEN, Ph.D., New York City.

'The Reversible Copper Oxide Plate': WOOLSEY McA. JOHNSON, Hartford, Conn.

'A Thermodynamical Note on the Theory of the Edison Battery': E. F. ROEBER, Ph.D., Philadelphia, Pa.

'Electrolysis of an Aqueous Solution by Alternating Current': Professor JOS. W. RICHARDS, Ph.D., Lehigh University, Bethlehem, Pa.

'The Atom of Electro-chemistry': ARVID REUTERDAHL, Providence, R. I.

Saturday afternoon will be devoted to visits to places of interest.

THE GEOLOGICAL SOCIETY OF WASHINGTON.

AT the meeting of the Society on February 26, the first paper, by Mr. T. W. Vaughan, was entitled 'Earliest Tertiary Coral Reefs in the Antilles and United States.' Mr. Vaughan made a few remarks in order to indicate when, during Tertiary time, the physical conditions in the regions mentioned in the title of his communication first became suitable for the formation of coral reefs. A few species of reef-building genera occur in the Midway (basal Eocene) beds of Alabama, and taken as a whole the Eocene corals of the United States characterize only moderately deep or shallow water; but strictly speaking, no Eocene coral reefs are known in the United States. Reef-building genera occur in the Vicksburgian (Lower) Oligocene at Vicksburg, Mississippi. The temperature of the water was at least subtropical and the depth was not great, probably not too great for the formation of reefs; but some other condition, probably such as muddiness of water, prevented their formation. The Coral limestone at Salt Mountain, Alabama, is a coral reef limestone, but its precise stratigraphic position has not been determined. It is either uppermost Lower Oligocene or basal Upper Oligocene. The Upper Oligocene in the United States was initiated by an extensive development of coral reefs. They occur in southwestern Georgia along the Flint River, the Tampa silex beds of Tampa, Florida, and at numerous other localities in Florida. The fauna is rich in genera, species and individuals. Reefs of the same age are very abundant in the Antilles. They occur in Cuba in the vicinity of Havana, Matanzas, Santiago and other places. They probably are present in the island of Haiti. Other islands in which Upper Oligocene rocks exist are Porto Rico, Antigua and Arube (Dutch West Indies).

There are no Miocene coral reefs in the United States, the temperature of the water being too cold. The species of corals known grew in water only a few fathoms in depth. It is not at present known whether or not Miocene reefs existed in the West Indies. Apparently during Miocene time the Antilles stood much higher than at present; therefore

if any did exist they would at present be submerged.

Pliocene reefs were extensively developed along the Florida coast, for instance, along the Caloosahatchee River. The genera of the Pliocene corals are the same as those at present living in the Floridian and Antillean seas, but often there are appreciable specific differences between the Pliocene and recent representatives of the same genus.

Mr. Bailey Willis spoke on the 'Conditions of Overthrust in the Northern Rockies.' After restating the facts relating to the overthrust of Algonkian strata upon Cretaceous with a displacement of more than seven miles, along the eastern flank of the northern Rocky Mountains in northwestern Montana, Mr. Willis presented a hypothesis of origin and development of this structure. It is assumed that in Cretaceous time Algonkian strata in this region were essentially flat, and in consequence of subsidence were buried under Dakota and Pierre sediments, with a shore line not far from the position of the present mountain range. Algonkian strata beneath the marine area being depressed and beneath the land area being raised, they were bent parallel to the general trend of the shore. When later the strata were compressed, the initial bend determined an anticline in this same position. Erosion of the arch cut deeply into Algonkian beds and left the edges exposed and free to move. Continued compression resulted in their being thrust upward and northeastward upon the eroded surface, until Algonkian limestones came to rest upon Cretaceous areas. The structure closely resembles the Rome fault, Georgia. The date of development is inferred to have been early Tertiary.

Mr. F. E. Matthes presented a paper on 'Glacial Erosion in the Northern Rockies.' The range was shown to have been deeply dissected before the advent of the glaciers. The valleys were nearing maturity and had low gradients; the glaciers which subsequently occupied them had therefore but little fall from their sources to their distal ends. They moved slowly and were of considerable thickness. The lengths of the various trunk glaciers were small in proportion to the large névé areas which they drained.

The frequent occurrence of many valleys radiating from one point was shown on the map. The effect of this arrangement upon the valley glaciers was shown to have been a general retardation of their flow and a consequent increase in thickness above their junction. Some attained a thickness of over 3,000 feet in some parts of their course.

The radiating system of ravines at the heads of valleys was shown to be particularly favorable to the development of cirques. At least two sets of cirques at different elevations are found in these mountains, indicating oscillations of the névé line to as low as 6,000 feet altitude.

The definitions of the snow line as given in three text-books now in use were compared and found to be greatly at variance with each other. A new definition was favored in which the topographic element is given due weight, and which makes the snow line virtually coincident with the névé line as found on glaciers.

The tendency of glaciers to flatten the grades of their channels, beginning at the upper ends, was shown to be productive of the step-like profiles of glaciated valleys. The cause of this tendency was sought in certain motions in the interior of the glaciers, the explanation of which was not attempted.

The widening of the valleys by cliff recession was emphasized as an important factor in producing discordance between valleys. Discordance was shown to be produced by (1) deepening of main valley, (2) widening of main valley; and to be diminished by (3) deepening of side valley. Cases were pointed out on the contour maps of valleys meeting with discordances ranging between 300 and 1,500 feet; also of several meeting with perfect accordance. Nor were the discordances always in inverse ratio to the drainage areas of the respective valleys.

The conclusion was reached that in no case could the discordance of a side valley be taken as a measure of the deepening of the main valley.

ALFRED H. BROOKS,
Secretary.

NEW YORK ACADEMY OF SCIENCES.

PUBLIC LECTURE.

On February 26 a public lecture was presented under the auspices of the Section of Biology, by Professor Bashford Dean, of Columbia University, entitled 'Journeyings of a Naturalist through Japan and the Philippines.'

Professor Dean referred to the zoological relations of the Japanese archipelago with the adjacent continent on the one hand, and with the island series on the other—*i. e.*, (1) the Aleutian, (2) through the Bonin Islands with the region of New Guinea, and (3) through the Liu Chiu Islands with Formosa and the Philippines. The importance of the line of Blakiston separating the Hokkaido from the southern islands was emphasized.

Especial attention was called to the favorable facilities for zoological work which are offered in the region of Misaki, near the mouth of the Bay of Tokyo, and to the work of the Marine Laboratory of the Imperial University in this region. Dr. Dean had an opportunity of examining the centers of animal artificialization, an art in which the Japanese have been so eminently successful. Especially praiseworthy is the method of oyster-culture practiced in the Inland Sea near Hiroshima; hardly less interesting were the establishments in which varieties of gold fish are propagated; and even more striking were those for the cultivation of the breed of Tosa fowls, in favorable specimens of which the tail feathers attain the great length of fifteen feet. Success in the maintenance of this breed appears to be due to the selection of those fowls in which moulting occurs irregularly, and the effort is made to entirely suppress the moult in that region of the fowl where long feathers are to be produced. In referring to a journey in the Philippines, Professor Dean described many interesting experiences, particularly those at Maujuyod, where living specimens of *Nautilus* were obtained.

Henry E. Crampton,
Secretary.

THE ACADEMY OF SCIENCE OF ST. LOUIS.

At the meeting of the Academy of Science of St. Louis on the evening of February 17, Dr. Gellert Alleman, of Washington University, delivered an address on 'The Chemical Constitution and the Manufacture of Portland Cements.' The growth of the cement industry was treated, the various steps of development being shown by lantern slides illustrating past and present types of machinery employed in its manufacture. Several slides were shown giving tabulated results of a number of analyses of different commercial Portland cements.

Mr. Charles Espenschied read a letter from Mr. Seymour Carter, of Hastings, Minnesota, in which was described a method of Professor Anderson, of Columbia University, by which it was stated that cereals could be directly transformed to food-stuffs. The process consists of enclosing the cereal to be treated in a hermetically sealed vessel and subjecting it to a temperature of about 450° F. for a certain time, and immediately thereafter opening the vessel, when it is found that the grains expand to six or eight times their normal size. The inventor states that the process does not alter the composition of the cereal. Samples of several cereals treated in this manner were shown.

Two persons were elected to active membership.

William Trelease,
Recording Secretary.

THE TORREY BOTANICAL CLUB.

At the meeting of the Club on January 29, the first paper was by Dr. Britton, entitled, 'Notes on the Crassulaceæ,' and is to appear in print, being a part of a contribution toward the projected 'Systematic Botany of North America.' Remarks followed by Dr. C. C. Curtis, Dr. Rydberg, Dr. Small, Dr. MacDougal and Mrs. Britton. The distribution of the Crassulaceæ was commented on, Dr. Britton speaking of the isolated colonies of high mountain species, which seem to have been continuously highly interbred, so producing highly specialized species.

The second paper, by Mr. F. S. Earle, entitled, 'New Genera of Fungi,' founded on rep-

resentatives from California and New Mexico, will soon appear in the Garden *Bulletin*.

Dr. Earle also exhibited a rosebush from under glass at the Garden, the roots of which have been attacked by a fungus now under examination and cultures of which were exhibited. The mycelium was abundant in the fibrous roots; also in the bark and cambium immediately above ground, and had caused sudden yellowing and dropping of the leaves.

Dr. MacDougal recalled the suggestion that potatoes are the result of fungal infection of the underground stem; it is said that no one has ever examined a potato tuber without finding fungus traces in it. In many cases of precocious blooming among both wild and cultivated plants, the cause is stimulus from similar infection.

Dr. MacDougal also exhibited specimens of two remarkable Alpine xerophytes from an altitude of 4,000 feet on New Zealand mountains, known as vegetable-sheep, *Raoulia* and *Haastia*, composites between which belongs *Gnaphalium* in order of affinity.

Dr. Rydberg spoke of a Rocky Mountain phlox with similar growth in cushion-like masses.

Mrs. Britton reported on the progress of her studies of a *Vittaria* collection made by Dr. Britton at St. Kitts, and exhibited drawings, and the present indication that two different specific names have been in use for different stages of the same life-history.

EDWARD S. BURGESS,
Secretary.

THE NORTHEASTERN SECTION OF THE AMERICAN CHEMICAL SOCIETY.

THE regular monthly meeting of the Section was held on February 27 in the physics lecture room of the Massachusetts Institute of Technology, Professor L. P. Kinnicutt presiding. Professor Henry P. Talbot addressed the Society on 'The Recorded History of the Members of the Argon Group.' The general history, the methods of isolation and identification of these gases, together with their physical properties, and their position in the periodic system were all carefully reviewed up to the present time.

HENRY FAY,
Secretary.

THE ONONDAGA ACADEMY OF SCIENCE.

THE Academy met in the historical rooms in Syracuse on Friday, Feb. 21, 1902. Dr. W. M. Beauchamp gave the presidential address on the 'Peopling of Early America.' He gave a résumé of the early investigations, touching briefly on the different theories proposed, and emphasizing the fact that the answer to the problem lies in the researches into the languages, customs and manners of the present nations and the archeological remains. Dr. Beauchamp gave many interesting items from his extended observations on the native tribes of New York State.

T. C. HOPKINS,
Corresponding Secretary.

DISCUSSION AND CORRESPONDENCE.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: ANTHROPOLOGY.

THE fifty-first meeting of the American Association for the Advancement of Science will be held at Pittsburgh, Pa., on June 28 and July 3, 1902. Mr. Stewart Culin, of the University of Pennsylvania, will preside over the Section of Anthropology.

Anthropologists are cordially invited to attend and contribute papers upon subjects connected with their fields of research. Several members of the Section have informally expressed the desire that some special effort should be made by the museum and field workers of the Section to present papers on the collections of importance with which they are familiar.

In order that a preliminary sectional program may be distributed in advance of the meeting, titles of communications should be sent to the secretary as soon as possible. Abstracts of papers, or the papers themselves, may be sent later at the convenience of the authors, who are reminded that no title will appear in the final program until the paper, either in full or in abstract, has been passed upon by the sectional committee.

HARLAN I. SMITH,
Secretary of Section H, Anthropology.

AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK.

FELIS OREGONENSIS RAF. AGAIN!

In his recent 'Revision of the Pumas' (*Proc. Wash. Acad. Sci.*, pp. 577–600), Dr. C. Hart Merriam devotes over a page to the inapplicability of the above name and to censuring my action in bringing it forward to replace *F. hippolestes olympus* given by himself in 1897. He states that it is a 'fallacious interpretation of our principles of nomenclature' to replace a name well characterized and accompanied by definite type and locality, by an older one deficient in these respects. This statement will, I think, be questioned by many zoologists who have erred in this way more than I.

Personally, I would be only too glad to throw out of consideration all the names proposed by Rafinesque and others of his time, as it would save us a deal of trouble, but if we recognize the principles of priority I see no excuse for such action, and such questions as the present one resolve themselves entirely into a consideration of the applicability of the older name.

This is largely a matter of individual opinion and in the absence of any tribunal for the consideration of a uniform nomenclature for our mammals individual preference will prevail. However a few words regarding Dr. Merriam's stand may not be out of place.

In Rafinesque's first paper he undoubtedly has in mind the *Felis concolor* group, that will be admitted on all hands.

In his second paper he names a variety of the puma (as mentioned in the first paper) from northwestern United States (Oregon by implication). The absence of a definite type locality in no way invalidates the name if otherwise satisfactorily diagnosed. We have many names now in use with just as vague type localities.

The description is very brief, but as good as many other early diagnoses and to my mind clearly indicates the same animal later characterized by Dr. Merriam. Moreover, I do not think it is 'grossly incorrect.'

Rafinesque says 'Dark brown, nearly black on the back, belly white.'

Dr. Merriam says 'Dark rufous brown, darkest along middle of back, backs of ears black, tip of tail blackish, breast and inguinal region soiled whitish, anterior part of throat white.'

Rafinesque had no 'manual of colors' and was of course not as exact as our present-day systematists, but it seems to me that his description is sufficiently accurate.

As to Dr. Merriam's argument that he probably never saw a specimen of the animal, we have positively no evidence one way or the other, and the fact does not affect the validity of the name nor do Dr. Merriam's further remarks about the other unrecognized cats that Rafinesque speaks of. The descriptions of *Cervus macrourus* and *C. hemionus* of the same author which are recognized and adopted by Dr. Merriam are associated with a lot of unidentifiable descriptions, and are admittedly based upon descriptions of travelers, while the diagnoses are no better than that of *Felis oregonensis*. If one stands, so should the others, in my estimation.

I might add that, so far as I am aware, every one who has written on this puma since my note appeared in Science has followed my views, even Dr. Merriam himself, who adopted the name *oregonensis* without comment, in his 'Biological Survey of Mt. Shasta' (p. 104).

As to the statement that no name based on hearsay accounts of travelers would be accepted if published to-day, we might suggest some recent cases that come pretty near to this, such as *Equus johnsoni*, which was based upon hearsay accounts of native Africans and two strips of skin, and *Macrias amissus* (Science, December 13), on a photograph and regretful recollections of a fish that was lost overboard after having been captured!

Witmer Stone.
Academy of Natural Sciences,
Philadelphia, Pa.

A VERY SENSITIVE THERMOSTAT.

For many forms of scientific investigation constancy of temperature is required. Such constancy may be secured, within a few hundredths of a degree, by several types of thermostat. For certain inquiries undertaken by the writer relative to the so-called 'critical' phenomena of liquids and gases, a much greater degree of accuracy was necessary.

To meet this demand, a thermostat was devised, of which a description will shortly appear in the *Journal of Physical Chemistry*. The regulator of this instrument functionates so perfectly that the temperature can be kept continuously at the same thousandth of a degree for hours at a time. It is so constructed moreover as to be capable of adjustment, within one or two hundredths of a degree, to any desired temperature over a range of about fifty degrees.

The most important factors which make such fineness of regulation possible are the following:

1. An extremely efficient circulation in the bath, which eliminates all local differences of temperature large enough to be readable.
2. Such a construction of the regulator that the expansive medium feels each minutest change of temperature and reacts promptly to it.
3. Provision for supplying the bath at all times with just the amount of heat needed, and no more. The regulation does not consist in alternately admitting and shutting off the inflow of heat, but in a 'throttling' of the same.

The extreme accuracy of function mentioned above is naturally obtained only when the thermostat is shielded from sudden changes of radiation. But excellent results are possible without such protection. Without the use of any insulation whatever, the bath can be held at a temperature of thirty or forty degrees within a hundredth of a degree.

W. P. BRADLEY.

WESLEYAN UNIVERSITY.

SCIENTIFIC NOMENCLATURE.

TO THE EDITOR OF SCIENCE: In SCIENCE for March 21, I find an article on 'Scientific Nomenclature,' by Mr. Frank W. Very, which concludes with the following words:

> Scientific descriptions remain unintelligible to the lazy man who hates to use the dictionary. They are free property to all who are willing to take this trouble.

On other pages of SCIENCE for March 21 (pp. 458 and 459), I find the words 'ecology' and 'ecological.' As I had never seen them before, I said to myself: 'Here is my chance to vindicate Mr. Very's judicious hint about the lazy man and the dictionary.' So I turned to the Century dictionary, but did not find ecology or ecological. I next had recourse to the new English dictionary of Murray, without success, and then to the new edition of Webster, published the present year. None of these contain the words above mentioned. Recourse to Liddell & Scott's Greek lexicon was equally unavailing. I am moved, therefore, to ask you for an explanation of this new term.

HORACE WHITE.

NEW YORK,
March 22, 1902.

[Ecology has doubtless been coined from the same word as economics, being the branch of zoology or botany that is concerned with the dwelling place or distribution of animals or plants. It will probably come as a shock to biologists to learn that this word is not to be found in recent dictionaries, as it is used in elementary books and courses. The word appears to be post-Darwinian; perhaps some reader can tell us when and where it was first used.—EDITOR.]

BOTANICAL NOTES.

A POPULAR BOOK ON TREES.

WHATEVER tends to popularize a knowledge of our trees is to be commended. Any book which induces a considerable number of people to give more attention to the structure and habits of trees deserves our hearty approval. It is true that too often these popular books are so full of blunders that the scientific man is constantly irritated as he runs over the pages, and as a consequence he is too often unable to see the great body of valuable matter hidden beneath the superficial errors. We have had within the last year or two a number of useful books dealing with plants of various kinds from mushrooms and ferns to wild flowering herbs, shrubs and trees. Now, another book is brought out by Knight and Millet, of Boston, under the title of 'Studies of Trees in Winter,' by Annie Oakes Huntington, with an introduction by Professor Sargent. The fact that so eminent a botanist

has thought it worth his while to write an introduction to the book at once bespeaks our good opinion. A glance at the pages is sufficient to show that in this we are not mistaken. There is, first, a short chapter giving such information as is necessary in the study of the tree in winter, followed by fourteen chapters on groups of trees, as 'The Horse-chestnut' (including also the Ohio buckeye), 'The Maples' (including seven species), 'The Ashes' (four species), 'The Walnuts and Hickories' (six species), etc. The illustrations are exquisite, consisting of 'half-tone' reproductions of characteristic photographs. The colored plates are especially fine, that of a cross-section of an oak trunk being really so perfect that one must run his hand over the plate to convince himself that it is not an actual section of the wood. While the book is printed and bound in a style quite too elegant for a text-book for schools, the subject matter is well adapted for such usage. A less expensive edition for schools should be brought out by the publishers, and in such form it should have wide use in the public schools.

GATTINGER'S FLORA OF TENNESSEE.

It speaks well for a state when its legislature authorizes the publication of a book on technical botany. This was done a little less than a year ago by the legislature of Tennessee in 'an act for the acceptance by the state of a work on botany prepared by Dr. A. Gattinger, and to make an appropriation for its publication and distribution.' The result is before us in the form of a neatly printed book of nearly three hundred pages. That the pages are marred by too many typographical slips is not the fault of the generous-minded men who made provision for its publication, nor of the venerable author, but of the inexperienced printer, to whom much of what he put into type must have been quite unintelligible. The book opens with about twenty pages of prefatory matter, in part historical; the remainder is devoted to a discussion of regional distribution of plants, and this is followed by about 160 pages devoted to an annotated list of the Pteridophyta and Spermatophyta of the state. Following this are about a hundred pages, entitled, the 'Philosophy of Botany,' including several papers of very unequal value. In the list of plants the modern system, as well as the modern nomenclature, is used, the latter being none other than that of the so-called 'Rochester Rules,' which he says he 'reluctantly adopted' after careful deliberation. This useful list is, therefore, another contribution to the more general use of the names recommended by the 'Rochester School' of systematic botanists, and is a sign of no small significance of the inevitable trend of botanical opinion and practice in this country.

The species noted are 2,218, of which 224 are Compositæ; 81, Labiatæ; 52, Umbelliferæ; 172, Malvaceæ; 251, Euphorbiaceæ; 103, Papilionaceæ; 83, Rosaceæ; 57, Cruciferæ; 61, Moraceæ; 124, Cyperaceæ; 223, Gramineæ; 15, Coniferæ; 61, Pteridophyta.

ENGLER'S PFLANZENREICH.

Heften 7 and 8 of this work have appeared within the past few weeks. Number 7 is devoted to the little group of water plants known as the Naiads (Naiadaceæ), and is from the hand of A. B. Rendle, of the British Museum. We have in this number a promise of what we may look for in the future, since this one has the general discussion in English, instead of in German, as has been the rule heretofore. It is quite novel to have a 'part' of a book in which three languages are used, the technical parts being in Latin, as usual, while some of the notes under the species are in German. In this paper the author restricts the family to the genus *Naias,* in which he recognizes thirty-two species. Number 8 takes up the maples (Aceraceæ), and the work is done by Dr. Ferdinand Pax, of the University of Breslau. Two genera are recognized, *Dipteronia,* a monotypic Chinese genus, and *Acer,* the maples proper. The latter genus is divided into thirteen sections, and all told, 113 species are described. In accordance with the latest conceptions of generic lines the box-elders (*Negundo*) are included in *Acer.* It is interesting to note that Dr. Pax has adopted *A. saccharnum* L. as the name of the silver maple (instead of *A. dasycarpum* Ehrh.) and *A. sac-*

charum Marshall for the sugar maple (not *A. saccharinum* Wang). In both numbers the illustrations are of the high order of the preceding *Heften*.

CHARLES E. BESSEY.

THE UNIVERSITY OF NEBRASKA.

NOTES ON INORGANIC CHEMISTRY.

THE first two numbers of the *Zeitschrift für Electrochemie* for January contain an experimental investigation by F. Haber and R. Geipert on the preparation of aluminum. The authors used as a crucible a block of coal $245 \times 245 \times 175$ mm., the opening having a diameter of 113 mm. at the bottom, 138 mm. at the top, and 70 mm. deep. This crucible served as a kathode, and a rod of coal 66 mm. in diameter as anode. The bath consisted of an artificial cryolite containing somewhat less than the theoretical amount of sodium fluorid, and in this pure alumina was dissolved. The most favorable current was 3 ampères per square centimeter at 7 to 10 volts. Under these conditions the electrolysis proceeded as smoothly and regularly as in the ordinary electro-analytical precipitation of a metal. Although the density of the solid bath is slightly greater than that of aluminum, when fused it is slightly lower. If, however, too much alumina is dissolved in the bath, it becomes too dense and the aluminum, instead of sinking, floats, often short-circuiting the current. A higher percentage of aluminum fluorid than is present in natural cryolite is advantageous, as it renders the bath more fusible. The output varied from 50 to 55 per cent. of that theoretically required by the current. The aluminum prepared was of particularly pure quality, and in the opinion of the authors the production of the same quality on a large scale is possible by the use of pure materials and an anode low in ash. It was found necessary to add fluorid to the bath from time to time to replace that which is lost by a gradual volatilization.

The modern manufacture of tin foil is described by Rafael Granja in the *Journal* of the Society of Chemical Industry. Three varieties of tin foil are on the market: pure tin foil, composition foil, and Dutch leaf. The composition foil consists of lead, covered on both sides with a thin coating of tin, while the Dutch leaf is prepared from an alloy of tin with a few per cent. of a secret metallic composition. The grade of fineness of the foil is expressed by the number of square inches which a pound of the foil will cover. Thus the limit reached by the thinnest pure tin foil is 10,000, by composition foil 7,000, while Dutch leaf reaches 14,000 square inches. The manufacture of the foil, and also of the capsules for the tops of bottles, is fully described in the paper.

FROM the Physiological Laboratory of the Veterinary High School of Vienna comes a contribution, which indirectly contributes to our knowledge of the occurrence of iodin in soils, and especially with reference to the question as to whether it is largely confined to those soils which are near the sea. On examining the thyroid glands of sheep from different Hungarian localities, Wohlmuth finds that the percentage of iodothyrin—0.2–0.35 per cent.—is approximately the same as that found by Baumann in German and French sheep, and that the iodothyrin contains about the same amount of iodin—3.2–3.3 per cent.—as that obtained by Baumann. The sheep from these far-inland localities must therefore have found in their food the necessary quantity of iodin for a normal amount of normal iodothyrin.

THE work of Liversidge on the crystalline structure of metallic nuggets has already been noticed in these columns. This work has been continued by the examination of a number of new specimens. The structure is studied by etching a polished surface of the metal. In nuggets from Lake Superior containing both silver and copper, it appears that the silver has been deposited upon the copper. Gold nuggets from the Klondyke present a structure and appearance quite different from those of any other locality. They are very pale in color, owing to the large quantity of silver present. An assay of two specimens gave only sixty-five per cent. of gold. In the case of silver and copper nuggets, as has been found with those of gold and platinum, there is every indication that the metal has been deposited

from solution, and there is nothing to indicate that the nuggets have undergone either igneous or hydrothermal fusion.

It is not often that there is an opportunity to determine the changes in a well water extending over a long period of years, but this has been done by W. W. Fisher in the case of the water of the Trafalgar Square well. He prints in a recent number of the *Analyst* an analysis just made of this water, comparing it with analyses made in 1848 and in 1857. These analyses show that the character of the water has not changed essentially, although the quantity of potassium salts has diminished quite decidedly. In this connection the author calls attention to the fact that alkaline waters are drawn not only from the chalk under the London clay, but also from other deep limestones, and draws the conclusion that the alkali salts present come from the chalk itself and not from percolation. In covered deposits where no natural drainage is possible, the chalk is found to contain soluble salts, distinct traces of sodium carbonate, chlorid and sulfate being found in chalk beneath London at a depth of 500 and 800 feet.

J. L. H.

RECENT ZOOPALEONTOLOGY.

FRITSCH'S 'FAUNA DER GASKOHLE UND DER KALKSTEINE DER PERMFORMATION, BÖMENS.'

Dr. Antoine Fritsch, of Prag, has recently issued a complete list of his publications extending back to the year 1851 and covering essentially the broad field of his zoological and paleontological observations. His most monumental work is on the primitive fishes, amphibians and reptiles of the Permian period described in a series of monographs under the title cited above, beginning in the year 1880.

The first monograph covers the long-bodied stegocephalian amphibians of the order Aistopoda; this was continued with the description of the short-bodied forms resembling the modern perennibranchiates in 1884. More advanced labyrinthodonts were described in 1885, the amphibian division of the fauna being concluded in 1887.

The second volume is mainly devoted to the lung fishes, or Dipnoi, and to the more primitive types of selachians. Most important of these types is the genus *Pleuracanthus* which bridges over the gap in fin-structure between the American genus *Cladoselache*, as described by Newberry and Dean, and the fin of the modern shark. This transition form completely disestablished the archipteryial theory of Gegenbaur and established the fin-fold theory of Thacher and Balfour. The other primitive selachians were concluded in 1893, and the great modern actinopterygian types corresponding to Agassiz's ganoids were covered in the parts which appeared during the succeeding two years.

The fourth volume, of which three parts have appeared between 1899 and the present time, is devoted to the insects of the Permian period, especially the myriopods and arachnoids. Finally, this monographic series is brought to a close in 1901 by the third part of the fourth volume which covers the crustaceans and molluscs. This series of monographs will constitute the greatest monument to its author. Also, those who visit Prag find there to their surprise that this Bohemian city contains one of the most beautiful zoological museums in the world, developed under the direction of this veteran zoologist.

H. F. O.

GRAVITY ON THE OCEAN.

The proceedings of the Academy of Sciences of Berlin of February 13, 1902, contain a paper by Professor F. R. Helmert on Dr. Hecker's determination of gravity on the Atlantic Ocean. In July and August, 1901, the International Geodetic Association entrusted Dr. Hecker, of the Potsdam Geodetic Institute, with the duty of making relative gravity observations on the Atlantic Ocean on a voyage between Hamburg and Bahia. The method employed was to determine the pressure of the atmosphere by means of a barometer and a hypsometer (boiling point thermometer). The barometric formula contains a term depending on the intensity of gravity at the place where the observation was made. The hypsometer is independent of this influ-

ence. The comparison of the results of the two methods affords a means of determining relative gravity with more or less accuracy. The results given are preliminary, but, according to Dr. Helmert, they are sufficient to indicate that gravity on the ocean where its depth is profound, between Lisbon and Bahia, is nearly normal. Dr. Helmert states that they furnish splendid confirmation of the hypothesis of Pratt in regard to the isostatic arrangement of the masses of the earth's crust. He states that taken in connection with the results of Nansen's pendulum observations on his North Polar Expedition this hypothesis, from now on, may be reckoned with as a fact at least in the sense of its being a general rule, and he believes that the radial anomalies of the geoid in comparison with the mean ellipsoid will probably not exceed the limits of $\pm$ 100 meters previously suggested by him.

O. H. T.

BRYAN DONKIN.

THE English journals announce the death of Mr. Bryan Donkin, a distinguished engineer and man of science to whom much credit is due for extensive and valuable work in the application of scientific methods in the development of the theory and the art of heat-engine design and construction. His research work has been extensive and continuous and his field of work, applied thermodynamics, mainly, afforded full play for all his energies.

Mr. Donkin was born in 1835, coming of a distinguished family of whom his father, John Bryan, his grandfather, Bryan Donkin, and the physician, Dr. Horatio Bryan Donkin, were famous members. He was educated at the University College of London and at the École Centrale des Arts et Métiers, in Paris, and later served an apprenticeship in the workshops of his uncle, at Bermondsey. He then went into business and was sent abroad to erect engines and the heavy machinery of paper-mills, and similar construction. He spent much time in Russia.

He was a partner in 1868 and the chairman of the corporation in 1889. About 1870, he became interested in the then rare opportunities of scientifically investigating the efficiencies of the heat-engines and presently made himself one of the leaders in promoting the modern scientific method in engineering and in researches relating to the subject. His influence in the promotion of the movement was exceedingly great and correspondingly useful. He was probably the first to make a complete balance-sheet exhibiting the receipts and expenditures of energy, in the operation of the steam-engine, in such manner as to reveal precisely the extent and the method of distribution of the stream of energy entering the system, its separation into the various currents flowing through the engine and its final disposition as useful and as wasted energy, and the resultant efficiency of the system.

He studied the effects of 'cylinder condensation' and of the two correctives of that serious form of wasted energy, superheating and steam-jacketing, and invented the 'revealer' to reveal the then mysterious changes occurring in the interior of the engine-cylinder. He established many important facts and laws of thermodynamic operations and thermal action, and was a very earnest advocate of all really sound movements in the direction of economic progress.

He wrote extensively on the subject which came to be his specialty and some of his papers are regarded as among the classics of that department of literature. He published a treatise on gas-engines which has now gone to a third edition and translated Diesel's 'Theory and Construction of the Rational Heat-Motor,' and, in 1898, issued a treatise on the steam-boiler. He was familiar with the French as with the German, and spent much time on the continent, studying the latest developments in his field, in all countries.

He was a vice-president of the British Institution of Mechanical Engineers, Watt medallist, Telford and Manby premium and prizeman of the Institution of Civil Engineers, a member of the Royal Institution and of a number of European associations and also of the American Society of Mechanical Engineers.

Mr. Donkin was famous for important and admirable professional work, both in construction and in research, was known in all

countries as a great writer and student and scholar, and, among his friends and acquaintances, was recognized as a man of genius and of heart, of perfect frankness and integrity, as well as of delightful personality. He was very extensively acquainted, at home and abroad. His death will be regretted by his numerous acquaintances, and by every one familiar with his work, and will be mourned long and sincerely by all who had the good fortune to be numbered among his personal friends. R. H. T.

SCIENTIFIC NOTES AND NEWS.

By order of the president, the spring meeting of the Council of the American Association for the Advancement of Science will be held in the Cosmos Club, Washington, D. C., on Thursday, April 17, 1902, at 4:30 P.M.

Edinburgh University will confer its LL.D. on President J. G. Schurman, of Cornell University, and on Principal A. W. Rücker, of London University.

Dr. Julius Kuehn, professor of agriculture at the University at Halle and director of the Agricultural Institute, has been elected a corresponding member of the Paris Academy of Sciences.

The Russian Geographical Society has awarded its Constantin medal to the geologist, K. J. Bogdanowitsch; the Semenoff medal to Dr. Eduard Suess, professor of geology in the University of Vienna, and the Przewalsky medal to the zoologist, Professor Zarudnyi.

Professor C. R. Barnes, of the University of Chicago, sailed for Europe March 22, and will spend nine months in visiting the botanical centers.

Dr. D. T. MacDougal has returned from Arizona and Sonora with an extensive collection of giant cacti and other large xerophytic plants, which are being installed in the horticultural houses of the New York Botanical Garden. Dr. MacDougal characterizes the recent sensational announcement in the daily press concerning the extermination of the tree cactus (*Cereus giganteus*) as being utterly without foundation.

Professor Tylor has given in his resignation of the office of keeper of the University Museum, Oxford, to which he was nominated on the death of the late Professor Henry Smith, who had succeeded Professor Phillips, the first occupant of the post, on the opening of the museum in 1857. Professor Tylor will continue to hold the readership in anthropology, to which he was appointed in 1884.

Dr. Earl Lintner, professor in the Technical Institute at Munich, has been made director of the scientific station for the study of brewing in the same city.

Dr. Louis Cobbett and Dr. E. S. St. Barbe Sladen have been appointed by the Royal Commission on Tuberculosis to assist in the experimental work of the commission to be carried out at Stansted. They will reside at the farms and devote the whole of their time to the investigations of the commission.

Owen's College, Manchester, celebrated its jubilee on March 12 and 13. Among those who presented addresses were Professor Becquerel, representing the Paris Academy of Sciences, and Professor Breymann, representing the Bavarian Academy of Sciences.

Mr. Creswell Shearer, of Trinity College, has been nominated to occupy the table at the Zoological Station at Naples, maintained by Cambridge University.

The Smith prizes at Cambridge University have been adjudged as follows: T. H. Havelock, B.A., St. John's College, for his essay 'On the Distribution of Energy in the Continuous Spectrum'; and J. E. Wright, B.A., Trinity College, for his essay, 'Singular Solutions of Differential Equations with Known Infinitesimal Transformations.'

The Department of Astronomy of Columbia University announces two lectures open to the public. On April 8 at 3:30 P.M. Mr. Percival Lowell will lecture on 'Modern Mars,' and on April 16 at the same hour, Dr. S. A. Mitchell will lecture on the recent eclipse expedition.

The Raoult memorial lecture of the Chemical Society, of London, was delivered by Professor van 't Hoff on March 26, in the lecture theater of the Royal Institution.

At a meeting of the members of St.

Thomas's Hospital and Medical School, London, it was decided that steps should be taken to institute a permanent memorial of the connection of the late Sir William MacCormac with the institution. A bust of the eminent surgeon will be placed in the central hall of the hospital, and if the amount of money collected should be in excess of the sum requisite for the bust, some further memorial will be established.

DR. THOMAS CONDON, professor of geology in the University of Oregon, celebrated his eightieth birthday on March 3.

MRS. MARY L. PULSIFER AMES, a writer on botany, has died at San Jose, Cal., aged fifty-seven years.

MR. HENRY HITCHCOCK, a prominent lawyer of St. Louis and one of the trustees of the Carnegie Institution, died on March 15, aged seventy-three years.

PROFESSOR MAXWELL SIMPSON, F.R.S., died on February 26, at the age of eighty-seven years. He had carried forward important researches on organic chemistry, and was for twenty years professor of chemistry in Queen's College, Cork.

PROFESSOR IVAN MUSCHKETOFF, known for his contributions to physical geography, has died at the age of fifty-two years.

THE death is announced of Major-General Pewzoff, known for his explorations in Central Asia, Mongolia and Tibet.

THE accounts of the executors of the late Judge Chas. P. Daly have been filed. It appears that the New York Botanical Garden will receive about $50,000 from his estate.

THE Civil Service Commission calls attention to an examination on April 22 for the position of assistant anthropologist in the Philippines at a salary of $2,400 per annum. At the same time there will be an examination for the position of aid in the Division of Physical and Chemical Geology, U. S. National Museum, at a salary of $1,200 per annum, and for the position of preparateur in the Division of Statigraphic Paleontology at a salary of $720 a year.

AT the Columbia meeting of the Society for Plant Morphology and Physiology, a large group photograph, including all the members then present, was taken and will soon be ready for distribution. It is a platinum print by Falk, on a card 15x14 inches, appropriately lettered, and will cost about $3.50. Members and others wishing copies should send their orders immediately to Dr. Erwin F. Smith, Department of Agriculture, Washington, D. C.

THE newly-organized American Philosophical Association will hold its first meeting at Columbia University, New York City, on March 31 and April 1. Professor J. E. Creighton is president of the Association.

DR. JOHN S. BILLINGS has presented to the New York Botanical Garden his large collection of fungi. It contains much valuable and interesting material; besides numerous specimens collected by Dr. Billings in the vicinity of Washington, D. C., there is a nearly complete series of Ravenel's Texan collections; it is particularly rich in representatives of the Sphaeriales, and includes many specimens of types or of authenticated specimens from the herbaria of Ravenel, Curtis, Schweinitz, Fries, Berkeley, Broome and other older mycologists. The series of herbarium specimens of Mexican plants collected in the States of Jalisco, Mexico, Zacatecas and Lower California by Mr. Leon Diquet and presented to the Garden by the Duke de Loubat, has been mounted for the herbarium. Other noteworthy series of Mexican plants recently added are the J. G. Schaffner collection, secured through the purchase of the Vigener Herbarium, and especially rich in the flora of middle Mexico, particularly the state of San Luis Potosi, and the C. L. Smith collection, consisting of specimens from the region of the Isthmus of Tehuantepec and contiguous states in southern Mexico.

IN experiments on the diffusion of nuclei, Professor C. Barus has recently found that the nucleus from the same source diffuses into water vapor more than 100 times more rapidly than into benzol or other organic vapors, under otherwise like conditions. The rate in the latter case is .017 cm./sec. The important result follows that the nucleus depends for its size on the medium in which it is suspended.

The phenomena as a whole are closely analogous to the suspension of clay in water and in organic liquids, respectively. The particles are smallest in water or in water vapor, while they have grown to relatively enormous sizes in case of the other liquids or vapors.

MR. HENRY E. KOCH, of the Biological Department of the University of Cincinnati, has recently made a discovery which places color-photography upon a scientific basis. He has found that certain aniline dyes with which a sensitive plate or paper may be impregnated are sensitive to the light which changes the silver salts; the aniline dye changes to the color of the object which is being photographed. The natural color thus reproduced in the plate or film may be rendered permanent by a fixing process, in the same way that the black and white picture in the silver salt is rendered permanent by the fixing bath.

AT the February meeting of the Council of the American Institute of Electrical Engineers, the following resolution, reported by the Committee on Standardization, was unanimously accepted and adopted:

Whereas, The metric system of weights and measures offers very great advantages by its simplicity, consistency and convenience in every-day use, as well as in all engineering calculations and computations, and

Whereas, These advantages have already been demonstrated by the universal adoption and entirely successful use of the metric system in all civilized countries except Great Britain and the United States, and

Whereas, All the electrical units in universal use, such as the volt, ampere, ohm, watt, etc., are metric units, and

Whereas, The industrial use of these electrical units would be much facilitated by the general adoption of the metric system,

Resolved, That this committee unanimously recommends the introduction of the metric system into general use in the United States at as early a date as possible without undue hardship to the industrial interests involved.

Resolved, That this committee favors such legislation by Congress as shall secure the adoption of the metric system by each department of the National Government as speedily as may be consistent with the public welfare.

WE learn from *Nature* that the ninth meeting of the Australasian Association for the Advancement of Science was held at Hobart on January 8–16, under the presidency of Captain F. W. Hutton, F.R.S., the subject of whose presidential address was 'Evolution and its Teaching.' The presidents of the sections and the subjects of their addresses were as follows: Mr. R. W. Chapman (Astronomy, Mathematics, Physics and Mechanics), 'Tidal Theory and its Application'; Professor A. M. A. Mica-Smith (Chemistry and Mineralogy), 'The Study of the Chemistry of the Air, and Whither it has Led'; Professor T. S. Hall (Geology and Paleontology), 'The Possibility of Detailed Correlation of Australian Formations with those of the Mother Hemisphere'; Professor W. B. Benham (Biology), 'Earthworms and Paleo-geography'; Rev. Geo. Brown (Geography), 'The Pacific, East and West'; Mr. T. A. Coghlan (Economic and Social Science and Statistics), 'The Statistical Question'; Dr. W. E. Roth (Ethnology and Anthropology), 'On the Games, Sports and Amusements of the North Queensland Aboriginals'; Sir T. Fitzgerald (Sanitary Science and Hygiene), 'The Nature of Diseases'; Professor A. Wall (Mental Science and Education), 'Poetry as a Factor in Education'; Mr. P. Oakden (Architecture and Engineering), no title announced. Many papers were read in each of the sections, and the titles in the official program show that a large proportion was of wide scientific interest. The handbook prepared for the use of the members contains a short historical sketch of Tasmania, and essays on the natural history of the country.

A LARGE number of members of the Royal Society and others have addressed to King Edward the following petition:

That Whereas His Majesty King Charles II., in order to prove that His Majesty did 'look with favour upon all forms of Learning' and particularly 'Philosophical Studies,' and in order that such Learning and Studies should 'shine conspicuously' among his People, did by Charters granted in the 14th, 15th and 21st years of His Reign found the Royal Society for the promotion of such Learning and Studies.

And Whereas the progress of Learning and Philosophical Studies has been great, and scientific methods of inquiry have been applied to many

new fields of knowledge since the time of His Majesty King Charles II.

And Whereas Your Petitioners are of opinion that it is desirable that all the Intellectual forces of the Realm should be so organised as to promote the greatest advancement of Scientific Studies within the Empire

And Whereas a large and influential group of representatives of Studies connected with History, Philosophy and Philology have lately presented a petition to Your Majesty praying to be embodied under Royal Charter as an Academy or like institution

And Whereas Your Petitioners are of opinion that such incorporation can be most efficiently provided for in some relationship to the Royal Society

We Your Petitioners humbly pray that Your Majesty may be graciously pleased to cause an Inquiry to be made with a view of instituting a general and formal organisation of all the Studies depending upon Scientific Method now carried on similar to that inaugurated for the Philosophical Studies of the 17th century by the Charters of His Majesty King Charles II.

ACCORDING to the New York *Evening Post* Mr. Andrew Carnegie's recent gifts of libraries affect forty-two towns, as follows:

Reno, Nev.	$15,000	Yankton, S. D.	10,000
Baraboo, Wis.	12,000	Berlin, Ont.	15,000
Greensburg, Md.	15,000	Benton Harbor, Mich.	15,000
London, O.	10,000	Victoria, B. C.	50,000
Blue Island, Ill.	15,000	Little Falls, Minn.	10,000
Littleton, N. H.	15,000	Newton, Kans.	10,000
Paris, Ill.	18,000	Atlantic, Iowa	12,500
Maquoketa, Iowa	10,000	St. Thomas, Ont.	15,000
Redfield, S. D.	10,000	Iowa City, Iowa	25,000
Denver, Col.	200,000	Beatrice, Nebr.	20,000
Las Vegas, N.M.	10,000	Cedar Falls, Iowa	15,000
Goderch, Ont.	10,000	Dennison, Iowa.	10,000
Bozeman, Mont.	15,000	Hampton, Iowa.	10,000
Saratoga, N. Y.	10,000	Athol, Mass.	15,000
San Bernardino, Cal.	15,000	New Albany, Ind.	35,000
Danville, Ind.	10,000	Tipton, Ind.	10,000
Nakoma, Ind.	20,000	Mount Clemens, Mich	15,000
Santa Rosa, Cal.	20,000	ChicagoHeights, Ill.	10,000
Charlotte, Mich.	10,000	Waukesha, Wis.	15,000
Brazil, Ind.	20,000		
Fulton, N. Y.	15,000		
New Brunswick, N. J.	50,000		
Oskaloosa, Iowa	$20,000		

A RECENT enumeration gives a total of 1,476 preparations of the brain in the neurologic division of the museum of Cornell University. Of these 402 are from human adults; 207 from fetuses or embryos; 282 from apes, monkeys and lemurs; 400 from other mammals, and 185 from other vertebrates.

THE Italian Government has accepted the offer of a German syndicate to drain the Pontine marshes, which stretch between the mountains and the coast, with a breadth varying from six to eleven miles, at a cost of $1,000,000. The reclaiming of the marshes is expected to free Rome from malarias. The syndicate has exacted a thirty-year lease of the reclaimed land, which it intends to use for farming and garden purposes.

THE map descriptive of Niagara Falls and the river which formed a part of the United States Geological Survey at the Pan-American Exposition is of special interest. Like all the sheets prepared by the Geological Survey, the map shows in great detail the usual features contained on ordinary map sheets and in addition reveals the relief of the country by the use of contours, or lines passing through all points of equal altitude. Of special interest, however, is the short 'Physical History of the Niagara River,' printed on the reverse side of the map, which traces its life history and the work it has done in excavating the deep gorge below the falls in which are located the famous whirlpool and the rapids just above it. It is interesting to note that the stream is described as not one of the old rivers of the earth, but one of comparative youth. The text also discusses the well-known recession of the falls by which they are slowly eating their way, at the present rate of four or five feet a year, back toward Lake Erie, which they will eventually reach. It touches upon the probable age of the river, or the time which has been consumed in the making of the gorge, on the effect which the great ice sheet of the Glacial Period had in changing its course, and on other features in connection with it which are of unusual interest. The description is accompanied by a bird's-eye view of Niagara River, showing the features described in the text. The map is not only an accurate one of the section, but, with its descriptive features, forms an excellent means of studying some of the most striking problems in physical geography. It can be had on application to the Director of the Geolog-

ical Survey, Washington, at the usual price of five cents per sheet.

UNIVERSITY AND EDUCATIONAL NEWS.

The Duke of Loubat has given the Collège de France an annuity of $1,200 to found and maintain a professorship for the study of American antiquities. In 1899 he founded a similar professorship in the University of Berlin.

The Columbian University of Washington, D. C., has just completed plans and let contracts for the erection of a new hospital building and a new medical and dental school on H Street, N. W., between Thirteenth and Fourteenth. The buildings will be colonial in style. The hospital will have a frontage of 60 feet to the south, and the medical school building (50x144 feet) will be five stories high. Large new laboratories thoroughly equipped for modern work, well-lighted lecture and reading rooms will afford excellent facilities for medical and dental students.

The University of Cincinnati has received a donation of about $5,000 for the purchase of presses and other machines for the University of Cincinnati Press. Hereafter the University will do all its printing and will print the scientific publications and texts which are published by the teachers in all the departments of the University.

The regents of the University of Michigan have indorsed the action of the engineering faculty, making it obligatory for students to spend six months between the junior and senior years in practical work.

The faculty of McGill University has decided to ask the Dominion Government at the present session of Parliament to enact a law inaugurating a five years' course in medicine instead of four as at present.

The courses offered by the graduate school of Yale University are distributed as follows: Philosophy, 50; social science, history and law, 77; Semitic languages and biblical literature, 59; classical and Indo-Iranian philology, 59; modern languages, 65; physical and natural sciences, 81; mathematics, 29; fine arts, 4; music, 7; physical culture, 3.

The *Bulletin* of the University of the State of Missouri gives the number of professors and instructors who have attended different universities as follows: Harvard, 15; Yale, 1; Columbia, 2; Johns Hopkins, 8; Virginia, 5; North Carolina, 1; Georgia, 1; Michigan, 3; Wisconsin, 2; California, 1; Stanford, 2; Indiana, 1; Missouri, 22; Dartmouth, 2; Chicago, 5; Miami, 1; Minnesota, 1; Lake Forest, 2; Cincinnati, 1; Clark, 3; Cornell, 6; Williams, 1; Lehigh, 1; DePauw, 2; Ohio, 1; Trinity (Toronto), 1; McGill, 1; Heidelberg, 3; Ecole des Beaux Arts, 1; Paris, 5; Berlin, 10; Halle, 2; Munich, 2; Classical School at Athens, 2; Classical School at Rome, 1; Strassburg, 1; Leipzig, 2; Goettingen, 2; University of London, 2.

Of the three European fellowships conferred at Bryn Mawr College, one has been awarded to Miss Marie Reimer, A.B. (Vassar) for work in chemistry, and one to Miss Harriet Brookes, A.B. (McGill), for work in physics.

Mr. R. A. S. Redmayne has been appointed professor of mining in the University of Birmingham, and Mr. Thomas Turner, professor of metallurgy.

The Isaac Newton studentship, Cambridge University, of the value of £250 for the encouragement of study and research in astronomy and physical optics, open to bachelors of arts under the age of 25 years, has been awarded to Mr. T. H. Havelock, B.A., scholar of St. John's College.

Mr. J. S. Budgett, of Trinity College, has been elected to the Balfour Studentship at Cambridge University. The studentship is tenable for three years and the annual value is about $1,000.

Dr. Ernst Beckmann, professor of chemistry at the University at Leipzig, has been called to the newly established chair of chemistry at the University of Berlin.

Dr. J. Piccard, professor of chemistry at the University of Bâsle, and Dr. E. Buguion, professor of anatomy at the University of Lausanne, will this year retire from active teaching.